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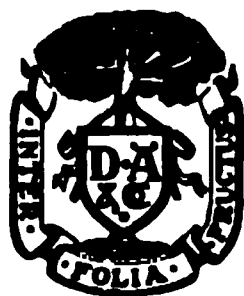
LIFE AND LETTERS OF
THOMAS HENRY HUXLEY

LIFE AND LETTERS
OF
THOMAS HENRY HUXLEY

BY HIS SON
LEONARD HUXLEY

IN TWO VOLUMES

VOL. I



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This was the note he struck in the appeal for intellectual sincerity and clearness which he made at the end of his New York *Lectures on Evolution*. The same note dominates that letter to his sister—a Southerner by adoption—which gives his reading of the real issue at stake in the great civil war. Slavery is bad for the slave, but far worse for the master, as sapping his character and making impossible that moral vigour of the individual on which is based the collective vigour of the nation.

The interest with which he followed the later development of social problems need not be dwelt on here, except to say that he watched their earlier maturity in America as an indication of the problems which would afterwards call for a solution in his own country. His share in treating them was limited to examining the principles of social philosophy on which some of the proposed remedies for social troubles were based, and this examination may be found in his *Collected Essays*. But the educational campaign which he carried on in England had its counterpart in America. It was not only that he was chosen to open the Johns Hopkins University as the type of a new form of education; there and elsewhere pupils of his carried out in America his methods of teaching biology, while others engaged in general education would write testifying to the influence of his ideas upon their own methods of teaching. But it must be remembered that nothing was further from his mind than the desire to found a school of thought. He only endeavoured as a scholar and a student to clear up his own thoughts and help others to clear theirs, whether in the intellectual or the moral world. This was the help he steadfastly hoped to give the people, that interacting union of intellectual freedom and moral discernment which may be furthered by good education and training, by precept and example, that basis of all social health and prosperity. And if, as he said, he would like to be remembered as one who had done his best to help the people, he meant assuredly not the people only of his native land, but the wider world to whom his words could be carried.

PREFACE TO THE ENGLISH EDITION

My father's life was one of so many interests, and his work was at all times so diversified, that to follow each thread separately, as if he had been engaged on that alone for a time, would be to give a false impression of his activity and the peculiar character of his labours. All through his active career he was equally busy with research into nature, with studies in philosophy, with teaching and administrative work. The real measure of his energy can only be found when all these are considered together. Without this there can be no conception of the limitations imposed upon him in his chosen life's work. The mere amount of his research is greatly magnified by the smallness of the time allowed for it.

But great as was the impression left by these researches in purely scientific circles, it is not by them alone that he made his impression upon the mass of his contemporaries. They were chiefly moved by something over and above his wide knowledge in so many fields—by his passionate sincerity, his interest not only in pure knowledge, but in human life, by his belief that the interpretation of the book of nature was not to be kept apart from the ultimate problems of existence; by the love of truth, in short, both theoretical and practical, which gave the key to the character of the man himself.

Accordingly, I have not discussed with any fulness the value of his technical contributions to natural science; I have not drawn up a compendium of his philosophical views. One is a work for specialists; the other can be gathered from his published works. I have endeavoured rather to give the public a picture, so far as I can, of the man himself, of his aims in the many struggles in which

he was engaged, of his character and temperament, and the circumstances under which his various works were begun and completed.

So far as possible, I have made his letters, or extracts from them, tell the story of his life. If those of any given period are diverse in tone and character, it is simply because they reflect an equal diversity of occupations and interests. Few of the letters, however, are of any great length; many are little more than hurried notes; others, mainly of private interest, supply a sentence here and there to fill in the general outline.

Moreover, whenever circumstances permit, I have endeavoured to make my own part in the book entirely impersonal. My experience is that the constant iteration by the biographer of his relationship to the subject of his memoir, can become exasperating to the reader; so that at the risk of offending in the opposite direction, I have chosen the other course.

Lastly, I have to express my grateful thanks to all who have sent me letters or supplied information, and especially to Dr. J. H. Gladstone, Sir Mountstuart Grant Duff, Professor Howes, Professor Henry Sidgwick, and Sir Spencer Walpole, for their contributions to the book; but above all to Sir Joseph Hooker and Sir Michael Foster, whose invaluable help in reading proofs and making suggestions has been, as it were, a final labour of love for the memory of their old friend.

CONTENTS

CHAPTER	PAGE
I. (1825-1842)	1
II. (1841-1846)	16
III. (1846-1849)	31
IV. (1848-1850)	44
V. (1850-1851)	60
VI. (1851-1854)	77
VII. (1851-1853)	94
VIII. (1854)	117
IX. (1855)	138
X. (1855-1858)	142
XI. (1857-1858)	154
XII. (1859-1860)	164
XIII. (1859)	178
XIV. (1859-1860)	188
XV. (1860-1863)	205
XVI. (1860-1861)	225
XVII. (1861-1863)	247
XVIII. (1864)	269
XIX. (1865)	283
XX. (1866)	294
XXI. (1867)	306
XXII. (1868)	316
XXIII. (1869)	330
XXIV. (1870)	346

x

LIFE OF PROFESSOR HUXLEY

CHAPTER	PAGE
XXV. (1871)	383
XXVI. (1872)	394
XXVII. (1873)	418
XXVIII. (1874)	436
XXIX. (1875-1876)	459
XXX. (1875-1876)	475
XXXI. (1876)	489
XXXII. (1877)	507
XXXIII. (1878)	520
APPENDIX	541

LIST OF ILLUSTRATIONS

	PACING PAGE
Portrait from a Daguerrotype made in 1846	<i>Frontispiece</i>
Facsimile of Sketch, "The Loves and Graces"	85
Portrait from a Photograph by Maull and Polyblank, 1857	160
No. 4 Marlborough Place—from the Garden. After a Water- colour Sketch by R. Huxley	412
Portrait from a Photograph by Elliott and Fry; Steel Engrav- ing in <i>Nature</i> , February 5, 1874	436



their original home in Cheshire. This home is represented to-day by a farm in the Broughton Hundred, about eight miles from Chester, called Huxley Hall. From this centre Huxleys spread to the neighbouring villages, such as Overton and Eccleston; Clotton and Duddon, Tattenhall and Wettenhall; others to Chester and Brindley near Nantwich. The southward movement carries some to the Welsh border, others into Shropshire. The Wettenhall family established themselves in the fourth generation at Rushall, and held property in Handsworth and Walsall; the Brindley family sent a branch to Macclesfield, whose representative, Samuel, must have been on the town council when the Young Pretender rode through on his way to Derby, for he was mayor in 1746; while at the end of the sixteenth century, George, the disinherited heir of Brindley, became a merchant in London, and purchased Wyre Hall at Edmonton, where his descendants lived for four generations, his grandson being knighted by Charles II in 1663.

But my father had no particular interest in tracing his early ancestry. "My own genealogical inquiries," he said, "have taken me so far back that I confess the later stages do not interest me." Towards the end of his life, however, my mother persuaded him to see what could be found out about Huxley Hall and the origin of the name. This proved to be from the manor of Huxley or Hodesleia, whereof one Swanus de Hockenhull was enfeoffed by the abbot and convent of St. Werburgh in the time of Richard I. Of the grandsons of this Swanus, the eldest kept the manor and name of Hockenhull (which is still extant in the Midlands); the younger ones took their name from the other fief.

But the historian of Cheshire records the fact that owing to the respectability of the name, it was unlawfully assumed by divers "losels and lewd fellows of the baser sort," and my father, with a fine show of earnestness, used to declare that he was certain the legitimate owners of the name were far too sober and respectable to have produced such a reprobate as himself, and one of these "losels" must be his progenitor.

Thomas Henry Huxley was born at Ealing on May 4,

That peculiarity has been passed on to me in full strength; it has often stood me in good stead; it has sometimes played me sad tricks, and it has always been a danger. But, after all, if my time were to come over again, there is nothing I would less willingly part with than my inheritance of mother-wit.

Restless, talkative, untiring to the day of her death, she was at sixty-six "as active and energetic as a young woman." His early devotion to her was remarkable. Describing her to his future wife he writes:—

As a child my love for her was a passion. I have lain awake for hours crying because I had a morbid fear of her death; her approbation was my greatest reward, her displeasure my greatest punishment.

I have next to nothing to say about my childhood (he continues in the Autobiography). In later years my mother, looking at me almost reproachfully, would sometimes say, "Ah! you were such a pretty boy!" whence I had no difficulty in concluding that I had not fulfilled my early promise in the matter of looks. In fact, I have a distinct recollection of certain curls of which I was vain, and of a conviction that I closely resembled that handsome, courtly gentleman, Sir Herbert Oakley, who was vicar of our parish, and who was as a god to us country folk, because he was occasionally visited by the then Prince George of Cambridge. I remember turning my pinafore wrong side forwards in order to represent a surplice, and preaching to my mother's maids in the kitchen as nearly as possible in Sir Herbert's manner one Sunday morning when the rest of the family were at church. That is the earliest indication of the strong clerical affinities which my friend Mr. Herbert Spencer has always ascribed to me, though I fancy they have for the most part remained in a latent state.

There remains no record of his having been a very precocious child. Indeed, it is usually the eldest child whose necessary companionship with his elders wins him this reputation. The youngest remains a child among children longer than any other of his brothers and sisters.

One talent, however, displayed itself early. The faculty of drawing he inherited from his father. But on the queer principle that training is either unnecessary to natural capacity or even ruins it, he never received regular instruction

into disgrace and he did not. We made it up, and thereafter I was unmolested. One of the greatest shocks I ever received in my life was to be told a dozen years afterwards by the groom who brought me my horse in a stable-yard in Sydney that he was my quondam antagonist. He had a long story of family misfortune to account for his position; but at that time it was necessary to deal very cautiously with mysterious strangers in New South Wales, and on inquiry I found that the unfortunate young man had not only been "sent out," but had undergone more than one colonial conviction.

His brief school career was happily cut short by the break up of the Ealing establishment. On the death of Dr. Nicholas, his sons attempted to carry on the school; but the numbers declined rapidly, and George Huxley, about 1835, returned to his native town of Coventry, where he obtained the modest post of manager of the Coventry savings bank, while his daughters eked out the slender family resources by keeping school.

In the meantime the boy Tom, as he was usually called, got little or no regular instruction. But he had an inquiring mind, and a singularly early turn for metaphysical speculation. He read everything he could lay hands on in his father's library. Not satisfied with the ordinary length of the day, he used, when a boy of twelve, to light his candle before dawn, pin a blanket round his shoulders, and sit up in bed to read Hutton's *Geology*. He discussed all manner of questions with his parents and friends, for his quick and eager mind made it possible for him to have friendships with people considerably older than himself. Among these may especially be noted his medical brother-in-law, Dr. Cooke of Coventry, who had married his sister Ellen in 1839, and through whom he early became interested in human anatomy; and George Anderson May, at that time in business at Hinckley (a small weaving centre some dozen miles distant from Coventry), whom his friends who knew him afterwards in the home which he made for himself on the farm at Elford, near Tamworth, will remember for his genial spirit and native love of letters. There was a real friendship between the two. The boy of fifteen notes down

of diverse living constructions, and the modifications of similar apparatuses to serve diverse ends. The extraordinary attraction I felt towards the study of the intricacies of living structure nearly proved fatal to me at the outset. I was a mere boy—I think between thirteen and fourteen years of age—when I was taken by some older student friends of mine to the first *post-mortem* examination I ever attended. All my life I have been most unfortunately sensitive to the disagreeables which attend anatomical pursuits, but on this occasion my curiosity overpowered all other feelings, and I spent two or three hours in gratifying it. I did not cut myself, and none of the ordinary symptoms of dissection-poison supervened, but poisoned I was somehow, and I remember sinking into a strange state of apathy. By way of a last chance, I was sent to the care of some good, kind people, friends of my father's, who lived in a farmhouse in the heart of Warwickshire. I remember staggering from my bed to the window on the bright spring morning after my arrival, and throwing open the casement. Life seemed to come back on the wings of the breeze, and to this day the faint odour of wood-smoke, like that which floated across the farmyard in the early morning, is as good to me as the "sweet south upon a bed of violets." I soon recovered, but for years I suffered from occasional paroxysms of internal pain, and from that time my constant friend, hypochondriacal dyspepsia, commenced his half-century of co-tenancy of my fleshly tabernacle.

Some little time after his return from the voyage of the *Rattlesnake*, Huxley succeeded in tracing his good Warwickshire friends again. A letter of May 11, 1852, from one of them, Miss K. Jaggard, tells how they had lost sight of the Huxleys after their departure from Coventry; how they were themselves dispersed by death, marriage, or retirement; and then proceeds to draw a lively sketch of the long delicate-looking lad, which clearly refers to this period or a little later.

My brother and sister who were living at Grove Fields when you visited there, have now retired from the cares of business, and are living very comfortably at Leamington. . . . I suppose you remember Mr. Joseph Russell, who used to live at Avon Dassett. He is now married and gone to live at Grove Fields, so that it is still occupied by a person of the same name as when you knew it. But it is very much altered in appearance since

on September 29, 1840, it is continued for a couple of years, and concludes with some vigorous annotations in 1845, when the little booklet emerged from a three years' oblivion at the bottom of an old desk. Early as this journal is, in it the boy displays three habits afterwards characteristic of the man: the habit of noting down any striking thought or saying he came across in the course of his reading; of speculating on the causes of things and discussing the right and wrong of existing institutions; and of making scientific experiments, using them to correct his theories.

The first entry, the heading, as it were, and keynote of all the rest, is a quotation from Novalis:—"Philosophy can bake no bread; but it can prove for us God, freedom, and immortality. Which, now, is more practical, Philosophy or Economy?" The reference here given is to a German edition of Novalis, so that it seems highly probable that the boy had learnt enough of the language to translate a bit for himself, though, as appears from entries in 1841, he had still to master the grammar completely.

In science, he was much interested in electricity; he makes a galvanic battery "in view of experiment to get crystallized carbon. Got it deposited, but not crystallized." Other experiments and theorizing upon them are recorded in the following year. Another entry showing the courage of youth, deserves mention:—

"Oct. 5 (1840).—Began speculating on the cause of colours at sunset. Has any explanation of them ever been attempted?" which is supplemented by an extract "from old book."

We may also remark the early note of Radicalism and resistance to anything savouring of injustice or oppression, together with the naïve honesty of the admission that his opinions may change with years.

Oct. 25 (at Hinckley).—Read Dr. S. Smith on the Divine Government.—Agree with him partly.—I should say that a general belief in his doctrines would have a very injurious effect on morals.

Nov. 22.—. . . Had a long talk with my mother and father about the right to make Dissenters pay church rates — and

4. To prepare for the Matriculation Examination at London University which requires knowledge of:—
 - (a) Algebra—Geometry } did not begin to read for
 - (b) Natural Philosophy } this till April.
 - (c) Chemistry.
 - (d) Greek—Latin.
 - (e) English History down to end of seventeenth century.
 - (f) Ancient History.
 English Grammar.
5. To make copious notes of all things I read.

Projects completed—

1. Partly. 2. Not at all. 3 and 5, stuck to these pretty closely.
4. (e) Read as far as Henry III. in Hume.
 - (a) Evolution and involution.
 - (b) Refraction of light—Polarisation partly.
 - (c) Laws of combination—must read them over again.
 - (d) Nothing.
 - (f) Nothing.

I must get on faster than this. I *must* adopt a fixed plan of studies, for unless this is done I find time slips away without knowing it—and let me remember this—that it is better to read a little and thoroughly, than cram a crude undigested mass into my head, though it be great in quantity.

(This is about the only resolution I have ever stuck to—1845).

[Well do I remember how in that little narrow surgery I used to work morning after morning and evening after evening at that insufferably dry and profitless book, Hume's *History*, how I worked against hope through the series of thefts, robberies, and throat-cutting in those three first volumes, and how at length I gave up the task in utter disgust and despair.

Mackintosh's *History*, on the other hand, I remember reading with great pleasure, and also Guizot's *Civilisation in Europe*, the scientific theoretical form of the latter especially pleased me, but the want of sufficient knowledge to test his conclusions was a great drawback. 1845].

There follow notes of work done in successive weeks—June 20 to August 9, and September 27 to October 4.

Under the head of objective knowledge comes first Physics, including the whole body of the relations of inanimate unorganised bodies; secondly, Physiology. Including the structure and functions of animal bodies, including language and Psychology; thirdly comes History.

One object for which I have attempted to form an arrangement of knowledge is that I may test the amount of my own acquirements. I shall form an extensive list of subjects on this plan, and as I acquire any one of them I shall strike it out of the list. May the list soon get black! though at present I shall hardly be able, I am afraid, to spot the paper.

(A prophecy! a prophecy, 1845!).

April 1842 introduces a number of quotations from Carlyle's Miscellaneous Writings, "Characteristics," some clear and crisp, others sinking into Carlyle's own vein of speculative mysticism, *e.g.*

"In the mind as in the body the sign of health is unconsciousness."

"Of our thinking it is but the upper surface that we shape into articulate thought; underneath the region of argument and conscious discourse lies the region of meditation."

"Genius is ever a secret to itself."

"The healthy understanding, we should say, is neither the argumentative nor the Logical, but the Intuitive, for the end of understanding is not to prove and find reasons but to know and believe" (!)

"The ages of heroism are not ages of Moral Philosophy. Virtue, when it is philosophised of, has become aware of itself, is sickly and beginning to decline."

At the same time more electrical experiments are recorded; and theories are advanced with pros and cons to account for the facts observed.

The last entry was made three years later—

Oct. 1845.—I have found singular pleasure—having accidentally raked this Büchlein from a corner of my desk—in looking over these scraps of notices of my past existence; an illustration of J. Paul's saying that a man has but to write down his yesterday's doings, and forthwith they appear surrounded with a poetic halo.

But after all, these are but the top skimmings of these five

years' living. I hardly care to look back into the seething depths of the working and boiling mass that lay beneath all this froth, and indeed I hardly know whether I could give myself any clear account of it. Remembrances of physical and mental pain . . . absence of sympathy, and thence a choking up of such few ideas as I did form clearly within my own mind.

Grief too, yet at the misfortune of others, for I have had few properly my own; so much the worse, for in that case I might have said or done somewhat, but here was powerless.

Oh, Tom, trouble not thyself about sympathy; thou hast two stout legs and young, wherefore need a staff?

Furthermore, it is twenty minutes past two, and time to go to bed.

Büchlein, it will be long before my secretiveness remains so quiet again; make the most of what thou hast got.

CHAPTER II

1841-1846

THE migration to Rotherhithe, noted under date of January 9, 1841, was a fresh step in his career. In 1839 both his sisters married, and both married doctors. Dr. Cooke, the husband of the elder sister, who was settled in Coventry, had begun to give him some instruction in the principles of medicine as early as the preceding June. It was now arranged that he should go as assistant to Mr. Chandler, of Rotherhithe, a practical preliminary to walking the hospitals and obtaining a medical degree in London. His experiences among the poor in the dock region of the East of London—for Dr. Chandler had charge of the parish—supplied him with a grim commentary on his diligent reading in Carlyle. Looking back on this period, he writes:—

The last recorded speech of Professor Teufelsdröckh proposes the toast 'Die Sache der Armen in Gottes und Teufelsnamen' (The cause of the Poor in Heaven's name and ——'s.) The cause of the Poor is the burden of *Past and Present*, *Chartism*, and *Latter-Day Pamphlets*. To me . . . this advocacy of the cause of the poor appealed very strongly . . . because . . . I had had the opportunity of seeing for myself something of the way the poor live. Not much, indeed, but still enough to give a terrible foundation of real knowledge to my speculations.

After telling how he came to know something of the East End, he proceeds:—

I saw strange things there—among the rest, people who came to me for medical aid, and who were really suffering from nothing but slow starvation. I have not forgotten—am not

capacity should be caught and led to a position where it might be useful instead of dangerous to social order.

After some time, however, he left Mr. Chandler to join his second brother-in-law,* who had set up in the north of London, and to whom he was duly apprenticed, as his brother James had been before him. This change gave him more time and opportunity to pursue his medical education. He attended lectures at the Sydenham College, and, as has been seen, began to prepare for the matriculation examination of the University of London. At the Sydenham College he met with no little success, winning, besides certificates of merit in other departments, a prize—his first prize—for botany. His vivid recollections, given below, of this entry into the scientific arena are taken from a journal he kept for his fiancée during his absence from Sydney on the cruises of the *Rattlesnake*.

ON BOARD H.M.S. *RATTLESNAKE*, CHRISTMAS 1847.

Next summer it will be six years since I made my first trial in the world. My first public competition, small as it was, was an epoch in my life. I had been attending (it was my first summer session) the botanical lectures at Chelsea. One morning I observed a notice stuck up—a notice of a public competition for medals, etc., to take place on the 1st August (if I recollect right). It was then the end of May or thereabouts. I remember looking longingly at the notice, and some one said to me, "Why don't you go in and try for it?" I laughed at the idea, for I was very young, and my knowledge somewhat of the vaguest. Nevertheless I mentioned the matter to S.† when I returned home. He likewise advised me to try, and so I determined I would. I set to work in earnest, and perseveringly applied myself to such works as I could lay my hands on, Lindley's and Decandolle's *Systems* and the *Annales des Sciences Naturelles* in the British Museum. I tried to read Schleiden, but my German was insufficient.

For a young hand I worked really hard from eight or nine in the morning until twelve at night, besides a long hot summer's walk over to Chelsea two or three times a week to hear Lindley. A great part of the time I worked till sunrise. The

* John Godwin Scott.

† His brother-in-law.

I knew too well my own deficiencies to have been either surprised or disappointed at failure, and I made a point of shattering all involuntary "castles in the air" as soon as possible. My worst anticipations were realised. One day S. came to me with a sorrowful expression of countenance. He had inquired of the Beadle as to the decision, and ascertained on the latter's authority that all the successful candidates were University College men, whereby, of course, I was excluded. I said, "Very well, the thing was not to be helped," put my best face upon the matter, and gave up all thoughts of it. Lizzie, too, came to comfort me, and, I believe, felt it more than I did. What was my surprise on returning home one afternoon to find myself suddenly seized, and the whole female household vehemently insisting on kissing me. It appeared an official-looking letter had arrived for me, and Lizzie, as I did not appear, could not restrain herself from opening it. I was second, and was to receive a medal * accordingly, and dine with the guild on the 9th November to have it bestowed.

I dined with the company, and bore my share in both pudding and praise, but the charm of success lay in Lizzie's warm congratulation and sympathy. Since then she always took upon herself to prophesy touching the future fortunes of "the boy."

The haphazard, unsystematic nature of preliminary medical study here presented can not fail to strike one with wonder. Thomas Huxley was now seventeen; he had already had two years' "practice in pharmacy" as a testimonial put it. After a similar apprenticeship, his brother had made the acquaintance of the director of the Gloucester Lunatic Asylum, and was given by him the post of dispenser or "apothecary," which he filled so satisfactorily as to receive a promise that if he went to London for a couple of years to complete his medical training, a substitute should

* Silver Medal of the Apothecaries' Society, 9th November 1842. Another botanical prize is a book—*La Botanique*, by A. Richard—with the following inscription :—

THOMÆ HUXLEY
In Exercitatione Botanicēs
Apud Scholam Collegii Sydenhamiensis
Optime Merenti
Hunc librum dono dedit
RICARDUS D. HOBLYN, Botanicēs Professor.

Looking back (he says) on my "Lehrjahre," I am sorry to say that I do not think that any account of my doings as a student would tend to edification. In fact, I should distinctly warn ingenuous youth to avoid imitating my example. I worked extremely hard when it pleased me, and when it did not, which was a very frequent case, I was extremely idle (unless making caricatures of one's pastors and masters is to be called a branch of industry), or else wasted my energies in wrong directions. I read everything I could lay hands upon, including novels, and took up all sorts of pursuits to drop them again quite as speedily. No doubt it was very largely my own fault, but the only instruction from which I obtained the proper effect of education was that which I received from Mr. Wharton Jones, who was the lecturer on physiology at the Charing Cross School of Medicine. The extent and precision of his knowledge impressed me greatly, and the severe exactness of his method of lecturing was quite to my taste. I do not know that I have ever felt so much respect for anybody as a teacher before or since. I worked hard to obtain his approbation, and he was extremely kind and helpful to the youngster who, I am afraid, took up more of his time than he had any right to do. It was he who suggested the publication of my first scientific paper—a very little one—in the *Medical Gazette* of 1845, and most kindly corrected the literary faults which abounded in it, short as it was; for at that time, and for many years afterwards, I detested the trouble of writing, and would take no pains with it.

He never forgot his debt to Wharton Jones, and years afterwards was delighted at being able to do him a good turn, by helping to obtain a pension for him. But although in retrospect he condemns the fitfulness of his energies and his want of system, which left much to be learned afterwards, which might with advantage have been learned then, still it was his energy that struck his contemporaries. I have a story from one of them that when the other students used to go out into the court of the hospital after lectures were over, they would invariably catch sight of young Huxley's dark head at a certain window bent over a microscope while they amused themselves outside. The constant silhouette framed in the outlines of the window tickled the fancy of the young fellows, and a wag amongst them dubbed it with a name that stuck, "The Sign of the Head and Microscope."

a letter, drew a plan, enclosed the two in an envelope, and tremblingly betook myself on the following afternoon to the Royal Institution.

"Is Dr. Faraday here?" said I to the porter. "No sir, he has just gone out." I felt relieved. "Be good enough to give him this letter," and I was hurrying out when a little man in a brown coat came in at the glass door. "Here is Dr. Faraday," said the man, and gave him my letter. He turned to me and courteously inquired what I wished. "To submit to you that letter, sir, if you are not occupied." "My time is always occupied, sir, but step this way," and he led me into the museum or library, for I forget which it was, only I know there was a glass case against which we leant. He read my letter, did not think my plan would answer. Was I acquainted with mechanism, what we call the laws of motion? I saw all was up with my poor scheme, so after trying a little to explain, in the course of which I certainly failed in giving him a clear idea of what I would be at, I thanked him for his attention, and went off as dissatisfied as ever. The sense of one part of the conversation I well recollect. He said "that were the perpetual motion possible, it would have occurred spontaneously in nature, and would have overpowered all other forces," or words to that effect. I did not see the force of this, but did not feel competent enough to discuss the question.

However, all this exorcised my devil, and he has rarely come to trouble me since. Some future day, perhaps, I may be able to call Faraday's attention more decidedly. *Perge modo!* "*wie das Gestirn, ohne Hast, ohne Rast*" (*Das Gestirn in a midshipman's berth!*).

In other respects also his student's career was a brilliant one. In 1843 he won the first chemical prize, the certificate stating that his "extraordinary diligence and success in the pursuit of this branch of science do him infinite honour." At the same time, he also won the first prize in the class of anatomy and physiology. On the back of Wharton Jones' certificate is scribbled in pencil: "Well, 'tis no matter. Honour pricks me on. Yea, but how if honour prick me off when I come on? How then?"

Finally, in 1845 he went up for his M.B. at London University, and won a gold medal for anatomy and physiology, being second in honours in that section.

either the first time I attended him, as it was my duty to do, or for some weeks afterwards. I am afraid to think of the lengths to which my tongue may have run on the subject of the churlishness of the chief, who was, in truth, one of the kindest-hearted and most considerate of men. But one day, as I was crossing the hospital square, Sir John stopped me and heaped coals of fire on my head by telling me that he had tried to get me one of the resident appointments, much coveted by the assistant-surgeons, but that the Admiralty had put in another man. "However," said he, "I mean to keep you here till I can get you something you will like," and turned upon his heel without waiting for the thanks I stammered out. That explained how it was I had not been packed off to the West Coast of Africa like some of my juniors, and why, eventually, I remained altogether seven months at Haslar.

After a long interval, during which "Old John" ignored my existence almost as completely as before, he stopped me again as we met in a casual way, and describing the service on which the *Rattlesnake* was likely to be employed, said that Captain Owen Stanley, who was to command the ship, had asked him to recommend an assistant surgeon who knew something of science; would I like that? Of course I jumped at the offer. "Very well, I give you leave; go to London at once and see Captain Stanley." I went, saw my future commander, who was very civil to me, and promised to ask that I should be appointed to his ship, as in due time I was. It is a singular thing that during the few months of my stay at Haslar I had among my mess-mates two future Directors-General of the Medical Service of the Navy (Sir Alexander Armstrong and Sir John Watt-Reid), with the present President of the College of Physicians, and my kindest of doctors, Sir Andrew Clark.

A letter to his eldest sister, Lizzie, dated from Haslar May 24, 1846, shows how he regarded the prospect now opening before him.

. . . As I see no special queries in your letter, I think I shall go on to tell you what that same way of life is likely to be—my fortune having already been told for me (for the next five years at least). I told you in my last that I was likely to have a permanency here. Well, I was recommended by Sir John Richardson, and should have certainly had it, had not (luckily) the Admiralty put in a man of their own. Having a good impudent

was delighted when I told him about my appointment. Dim visions of strangely formed corpuscles seemed to cross his imagination like the ghosts of the kings in *Macbeth*.

What seems his head
The likeness of a nucleated cell has on.

The law's delays are proverbial, but on this occasion, as on the return of the *Rattlesnake*, the Admiralty seem to have been almost as provoking to the eager young surgeon as any lawyer could have been. The appointment was promised in May; it was not made till October. On the 6th of that month there is another letter to his sister, giving fuller particulars of his prospects on the voyage:—

MY DEAREST LIZZIE—At last I have really got my appointment and joined my ship. I was so completely disgusted with the many delays that had occurred that I made up my mind not to write to anybody again until I had my commission in my hand. Henceforward, like another Jonah, my dwelling-place will be the “inwards” of the *Rattlesnake*, and upon the whole I really doubt whether Jonah was much worse accommodated, so far as room goes, than myself. My total length, as you are aware, is considerable, 5 feet 11 inches, possibly, but the height of the lower deck of the *Rattlesnake*, which will be my especial location, is at the outside 4 feet 10 inches. What I am to do with the superfluous foot I cannot divine. Happily, however, there is a sort of skylight into the berth, so that I shall be able to sit with the body in it and my head out.

Apart from joking, however, this is not such a great matter, and it is the only thing I would see altered in the whole affair. The officers, as far as I have seen them, are a very gentlemanly, excellent set of men, and considering we are to be together for four or five years, that is a matter of no small importance. I am not given to be sanguine, but I confess I expect a good deal to arise out of this appointment. In the first place, surveying ships are totally different from the ordinary run of men-of-war. The requisite discipline is kept up, but not in the martinet style. Less form is observed. From the men who are appointed having more or less scientific turns, they have more respect for one another than that given by mere position in the service, and hence that position is less taken advantage of. They are brought more into contact, and hence those engaged in the surveying service almost proverbially stick by one another. To

apologise. Kind regards to the Doctor and kisses to the babbies. Write me a long letter all about yourselves.—Your affect. brother,

T. H. HUXLEY.

One more description to complete the sketch of his quarters on board the *Rattlesnake*. It is from a letter to his mother, written at Plymouth, where the *Rattlesnake* put in after leaving Portsmouth. The comparison with the ordinary quarters of an assistant-surgeon, and the shifts to which a studious man might be put in his endeavour to find a quiet spot to work in, have a flavour of Mr. Midshipman Easy about them to relieve the deplorable reality of his situation:—

You will be very glad to know that I am exceedingly comfortable here. My cabin has now got into tolerable order, and what with my books—which are, I am happy to say, not a few—my gay curtain and the spicy oilcloth which will be down on the floor, looks most respectable. Furthermore, although it is an unquestionably dull day I have sufficient light to write here, without the least trouble, to read, or even if necessary, to use my microscope. I went to see a friend of mine on board the *Recruit* the other day, and truly I hugged myself when I compared my position with his. The berth where he and seven others eat their daily bread is hardly bigger than my cabin, except in height—and, of course, he has to sleep in a hammock. My friend is rather an eccentric character, and, being missed in the ship, was discovered the other day reading in the maintop—the only place, as he said, sufficiently retired for study. And this is really no exaggeration. If I had no cabin I should take to drinking in a month.

It was during this period of waiting that he attended his first meeting of the British Association, which was held in 1846 at Southampton. Here he obtained from Professor Edward Forbes one of his living specimens of *Amphioxus lanceolatus*, and made an examination of its blood. The result was a short paper read at the following meeting of the Association,* which showed that in the composition of its blood this lowly vertebrate approached very near the invertebrates.

* “Examination of the Corpuscles of the Blood of *Amphioxus lanceolatus*,” *British Association Report*, 1847, ii. p. 95, and *Sci. Memoirs*, i.

CHAPTER III

1846-1849

It is a curious coincidence that, like two other leaders of science, Charles Darwin and Joseph Dalton Hooker, their close friend Huxley began his scientific career on board one of Her Majesty's ships. He was, however, to learn how little the British Government of that day, for all its professions, really cared for the advancement of knowledge.* But of the immense value to himself of these years of hard training, the discipline, the knowledge of men and of the capabilities of life, even without more than the barest necessities of existence—of this he often spoke. As he puts it in his Autobiography:—

Life on board Her Majesty's ships in those days was a very different affair from what it is now, and ours was exceptionally rough, as we were often many months without receiving letters or seeing any civilised people but ourselves. In exchange, we had the interest of being about the last voyagers, I suppose, to whom it could be possible to meet with people who knew noth-

* The key to this attitude on the part of the Admiralty is to be found in the scathing description in Briggs' *Naval Administration from 1827 to 1892*, p. 92, of the ruinous parsimony of either political party at this time with regard to the navy—a policy the results of which were only too apparent at the outbreak of the Crimean War. I quote a couple of sentences, "The navy estimates were framed upon the lowest scale, and reduction pushed to the very verge of danger." "Even from a financial point of view the course pursued was the reverse of economical, and ultimately led to wasteful and increased expenditure." Thus the liberal professions of the Admiralty were not fulfilled; its goodwill gave the young surgeon three and a half years of leave from active service; with an obdurate Treasury, it could do no more.

ing of firearms—as we did on the south coast of New Guinea—and of making acquaintance with a variety of interesting savage and semi-civilised people. But, apart from experience of this kind and the opportunities offered for scientific work, to me, personally, the cruise was extremely valuable. It was good for me to live under sharp discipline; to be down on the realities of existence by living on bare necessities: to find how extremely well worth living life seemed to be when one woke up from a night's rest on a soft plank, with the sky for canopy, and cocoa and weevilly biscuit the sole prospect for breakfast; and, more especially, to learn to work for the sake of what I got for myself out of it, even if it all went to the bottom and I along with it. My brother officers were as good fellows as sailors ought to be and generally are, but, naturally, they neither knew nor cared anything about my pursuits, nor understood why I should be so zealous in pursuit of the objects which my friends, the middies, christened "Buffons," after the title conspicuous on a volume of the *Suites à Buffon*, which stood on my shelf in the chart-room.

On the whole, life among the company of officers was satisfactory enough.* Huxley's immediate superior, John Thompson, was a man of sterling worth; and Captain Stanley was an excellent commander, and sympathetic withal. Among Huxley's messmates there was only one, the ship's clerk, who ever made himself actively disagreeable, and a quarrel with him only served to bring into relief the young surgeon's integrity and directness of action. After some dispute, in which he had been worsted, this gentleman sought to avenge himself by dropping mysterious hints as to Huxley's conduct before joining the ship. He had been treasurer of his mess; there had been trouble about the accounts, and a scandal had barely been averted. This was not long in coming to Huxley's ears. Furiously indignant as he was, he did not lose his self-control; but promptly

* The Assistant-Surgeon messed in the gun-room with the middies. A man in the midst of a lot of boys, with hardly any grown-up companions, often has a rather unenviable position; but, says Captain Heath, who was one of these middies, Huxley's constant good spirits and fun, when he was not absorbed in his work, his freedom from any assumption of superiority over them, made the boys his good comrades and allies.

We have come in for the purpose of effecting some trifling repairs, which, though not essential to the safety of the ship, will nevertheless naturally enhance the comfort of its inmates. This you will understand when I tell you that in consequence of these same defects I have had water an inch or two deep in my cabin, wish-washing about ever since we left Madeira.

We crossed the line on the 13th of this month, and as one of the uninitiated I went through the usual tomfoolery practised on that occasion. The affair has been too often described for me to say anything about it. I had the good luck to be ducked and shaved early, and of course took particular care to do my best in serving out the unhappy beggars who had to follow. I enjoyed the fun well enough at the time, but unquestionably it is on all grounds a most pernicious custom. It swelled our sick list to double the usual amount, and one poor fellow, I am sorry to say, died of the effects of pleurisy then contracted.

We have been quite long enough at sea now to enable me to judge how I shall get on in the ship, and to form a very clear idea of how it fits me and how I fit it. In the first place I am exceedingly well and exceedingly contented with my lot. My opinion of the advantages lying open to me increases rather than otherwise as I see my way about me. I am on capital terms with all the superior officers, and I find them ready to give me all facilities. I have a place for my books and microscope in the chart room, and there I sit and read in the morning much as though I were in my rooms in Agar Street. My immediate superior, Johnny Thompson, is a long-headed good fellow without a morsel of humbug about him—a man whom I thoroughly respect, both morally and intellectually. I think it will be my fault if we are not fast friends through the commission. One friend on board a ship is as much as anybody has a right to expect.

It is just the interval between the sea and the land breezes, the sea like glass, and not a breath stirring. I shall become soup if I do not go on deck. Temp. in sun at noon 86 in shade, 139 in sun. *N.B.*—It has been up to 89 in shade, 139 in sun since this.

March 28.—I see I concluded with a statement of temp. Since then it has been considerably better—140 in sun; however, in the shade it rarely rises above 86 or so, and when the sea or land breezes are blowing this is rather pleasant than otherwise.

I have been ashore two or three times. The town is like

In my last letter I think I mentioned to you that I had worked out and sent home to the President of the Linnæan Soc., through Capt. Stanley, an account of *Physalia*, or Portuguese man-of-war as it is called, an animal whose structure and affinities had never been properly worked out. The careful investigation I made gave rise to several new ideas covering the whole class of animals to which this creature belongs, and these ideas I have had the good fortune to have had many opportunities of working out in the course of our subsequent wanderings, so that I am provided with materials for a second paper far more considerable in extent, and embracing an altogether wider field. This second paper is now partly *in esse*—that is, written out—and partly *in posse*—that is, in my head; but I shall send it before leaving. Its title will be "Observations upon the Anatomy of the Diphydæ, and upon the Unity of Organisation of the Diphydæ and Physophoridæ," and it will have lots of figures to illustrate it. Now when we return from the north I hope to have collected materials for a much bigger paper than either of these, and to which they will serve as steps. If my present anticipations turn out correct, this paper will achieve one of the great ends of Zoology and Anatomy, viz. the reduction of two or three apparently widely separated and incongruous groups into modifications of the single type, every step of the reasoning being based upon anatomical facts. There! Think yourself lucky you have only got that to read instead of the slight abstract of all three papers with which I had some intention of favouring you.*

But five years ago you threw a slipper after me for luck on my first examination, and I must have you to do it for everything else.

At the Cape a stay of a month was made, from March 6 to April 10, and certain surveying work was done, after which the *Rattlesnake* sailed for Mauritius. In spite of the fact that the novelty of tropical scenery had worn off, the place made a deep impression. He writes to his mother, May 15, 1847:—

After a long and somewhat rough passage from the Cape, we made the highland of the Isle of France on the afternoon of the

* These papers are to be found in vol. i. of the *Scientific Memoirs* of T. H. Huxley, p. 9.

funeral urn supported on a pedestal, and as dilapidated as the rest of the affair. These dilapidations, as usual, are the work of English visitors, relic-hunters, who are as shameless here as elsewhere. I was exceedingly pleased on the whole with my excursion, and when I returned I made a drawing of the place, which I will send some day or other.

Since this I have made, in company with our purser and a passenger, Mr. King, a regular pedestrian trip to see some very beautiful falls up the country.

Leaving Mauritius on May 17, they prolonged their voyage to Sydney by being requisitioned to take more specie to Hobart Town, so that Sydney was not reached until July 16, eight months since they had had news of home.

The three months spent in this first visit to Sydney proved to be one of the most vital periods in the young surgeon's career. From boyhood up, vaguely conscious of unrest, of great powers within him working to find expression, he had yet been to a certain extent driven in upon himself. He had been somewhat isolated from those of his own age by his eagerness for problems about which they cared nothing; and the tendency to solitude, the habit of outward reserve imposed upon an unusually warm nature, were intensified by the fact that he grew up in surroundings not wholly congenial. One member alone of his family felt with him that complete and vivid sympathy which is so necessary to the full development of such a nature. When he was fourteen this sister married and left home, but the bond between them was not broken. In some ways it was strengthened by the lad's love for her children; by his grief, scarcely less than her own, at the death of her eldest little girl. Moreover they were brought into close companionship for a considerable time when, after his dismal period of apprenticeship at Rotherhithe—to which he could never look back without a shudder—he came to work under her husband. She had encouraged him in his studies; had urged him to work for the Botanical prize at Sydenham College; had brightened his life with her sympathy, and believed firmly in the brilliant future which awaited him—a

SYDNEY HARBOUR, *March 21, 1848.*

. . . I have deferred writing to you in the hope of knowing something from yourself of your doings and whereabouts, and now that we are on the eve of departing for a long cruise in Torres Straits, I will no longer postpone the giving you some account of "was ist geschehen" on this side of the world. We spent three months in Sydney, and a gay three months of it we had,—nothing but balls and parties the whole time. In this corner of the universe, where men of war are rather scarce, even the old *Rattlesnake* is rather a lion, and her officers are esteemed accordingly. Besides, to tell you the truth, we are rather agreeable people than otherwise, and can manage to get up a very decent turn-out on board on occasion. What think you of your grave, scientific brother turning out a ball-goer and doing the "light fantastic" to a great extent? It is a great fact, I assure you. But there is a method in my madness. I found it exceedingly disagreeable to come to a great place like Sydney and think there was not a soul who cared whether I was alive or dead, so I determined to go into what society was to be had and see if I could not pick up a friend or two among the multitude of the empty and frivolous. I am happy to say that I have had more success than I hoped for or deserved, and there are now two or three houses where I can go and feel myself at home at all times. But my "home" in Sydney is the house of my good friend Mr. Fanning, one of the first merchants in the place. But thereby hangs a tale which, of all people in the world, I must tell you. Mrs. Fanning has a sister, and the dear little sister and I managed to fall in love with one another in the most absurd manner after seeing one another—I will not tell you how few times, lest you should laugh. Do you remember how you used to talk to me about choosing a wife? Well, I think that my choice would justify even your fastidiousness. . . . I think you will understand how happy her love ought to and does make me. I fear that in this respect indeed the advantage is on my side, for my present wandering life and uncertain position must necessarily give her many an anxious thought. Our future is indeed none of the clearest. Three years at the very least must elapse before the *Rattlesnake* returns to England, and then unless I can write myself into my promotion or something else, we shall be just where we were. Nevertheless I have the strongest persuasion that four years hence I shall be married and settled in England. We shall see.

difference in *our years*, but she is *in fact* as much younger than her years as I am older than mine. Next, as to complexion she is exceedingly fair, with the Saxon yellow hair and blue eyes. Then as to face, I really don't know whether she is pretty or not. I have never been able to decide the matter in my own mind. Sometimes I think she is, and sometimes I wonder how the idea ever came into my head. Whether or not, her personal appearance has nothing whatever to do with the hold she has upon my mind, for I have seen hundreds of prettier women. But I never met with so sweet a temper, so self-sacrificing and affectionate a disposition, or so pure and womanly a mind, and from the perfectly intimate footing on which I stand with her family I have plenty of opportunities of judging. As I tell her, the only great folly I am aware of her being guilty of was the leaving her happiness in the hands of a man like myself, struggling upwards and certain of nothing.

As to my future intentions I can say very little about them. With my present income, of course, marriage is rather a bad look out, but I do not think it would be at all fair towards N. herself to leave this country without giving her a wife's claim upon me. . . . It is very unlikely I shall ever remain in the colony. Nothing but a very favourable chance could induce me to do so.

Much must depend upon how things go in England. If my various papers meet with any success, I may perhaps be able to leave the service. At present, however, I have not heard a word of anything I have sent. Professor Forbes has, I believe, published some of MacGillivray's letters to him, but he has apparently forgotten to write to MacGillivray himself, or to me. So I shall certainly send him nothing more, especially as Mr. MacLeay (of this place, and a great man in the naturalist world) has offered to get anything of mine sent to the Zoological Society.

In the paper mentioned in the letter of March 21, above ("On the Anatomy and Affinities of the Family of the Medusae"), Huxley aimed at "giving broad and general views of the whole class, considered as organised upon a given type, and inquiring into its relations with other families," unlike previous observers whose patience and ability had been devoted rather to "stating matters of detail concerning particular genera and species." At the outset, section 8 (*Sci. Mem.*, i. 11), he states—

I would wish to lay particular stress upon the composition of this (the stomach) and other organs of the Medusæ out of *two distinct membranes*, as I believe that it is one of the essential peculiarities of their structure, and that a knowledge of the fact is of great importance in investigating their homologies. I will call these two membranes as such, and independently of any modifications into particular organs, "foundation membranes."

And in section 56 (p. 23) one of the general conclusions which he deduces from his observations, is

That a Medusa consists essentially of two membranes inclosing a variously-shaped cavity, inasmuch as its various organs are so composed,

a peculiarity shared by certain other families of zoophytes. This is the point which that eminent authority, Professor G. J. Allman, had in his mind when he wrote to call my attention

to a fact which has been overlooked in all the notices I have seen, and which I regard as one of the greatest claims of his splendid work on the recognition of zoologists. I refer to his discovery that the body of the Medusæ is essentially composed of two membranes, an outer and an inner, and his recognition of these as the homologues of the two primary germinal leaflets in the vertebrate embryo. Now this discovery stands at the very basis of a philosophic zoology, and of a true conception of the affinities of animals. It is the ground on which Hæckel has founded his famous Gastræa Theory, and without it Kowalesky could never have announced his great discovery of the affinity of the Ascidians and Vertebrates, by which zoologists had been startled.

CHAPTER IV

1848-1850

THE whole cruise of the *Rattlesnake* lasted almost precisely four years, her stay in Australian waters nearly three. Of this time altogether eleven months were spent at Sydney, namely, July 16 to October 11, 1847; January 14 to February 2, and March 9 to April 29, 1848; January 24 to May 8, 1849; and February 14 to May 2, 1850. The three months of the first northern cruise were spent in the survey of the Inshore Passage—the passage, that is, within the Great Barrier Reef for ships proceeding from India to Sydney. In 1848, while waiting for the right season to visit Torres Straits, a short cruise was made in February and March, to inspect the lighthouses in Bass' Straits. It was on this occasion that Huxley visited Melbourne, then an insignificant town, before the discovery of gold had brought a rush of immigrants.

The second northern cruise of 1848, which lasted nine months, had for its object the completion of the survey of the Inner Passage as far as New Guinea and the adjoining archipelago. The third cruise in 1849-50 again lasted nine months, and continued the survey in Torres Straits, the Louisiade archipelago, and the south-eastern part of New Guinea. After this the original plan was to make a fourth cruise, filling up the charts of the Inner Passage on the east coast, and surveying the straits of Alass between Lombok and Sumbawa in the Malay Archipelago; then, instead of returning to Sydney, to proceed to Singapore and so home by the Cape. But these plans were

York also, a white woman was rescued who had been made prisoner by the blacks from a wreck, and had lived among them for several years. Here, too, Huxley and MacGillivray made a trip inland, and were welcomed by a native chief, who saw in the former the returning spirit of his dead brother.

Throughout the voyage Huxley was busy with his pencil, and many lithographs from his drawings illustrate the account of the voyage afterwards published. As to his scientific work, he was accumulating a large stock of observations, but felt rather sore about the papers which he had already sent home, for no word had reached him as to their fate, not even that they had been received or looked over by Forbes, to whom they had been consigned. As a matter of fact, they had not been neglected, as he was to find out on his return; but meanwhile the state of affairs was not reassuring to a man whose dearest hopes were bound up in the reception he could win for these and similar researches. Altogether, it was with no little joy that he turned his back on the sweltering heat of Torres Straits, on the great mountains of New Guinea, the Owen Stanley range, which had remained hidden from D'Urville in the *Astrolabe* to be discovered by the explorers on the *Rattlesnake*, and the far stretching archipelago of the Louisiades, one tiny island in which still bears the name of Huxley, after the assistant-surgeon of the *Rattlesnake*.

A few extracts from letters of the time will give a more vivid idea of what the voyage was like. The first is from a letter to his mother, dated February 1, 1849:—

. . . I suppose you have wondered at the long intervals of my letters, but my silence has been forced. I wrote from Rockingham Bay in May, and from Cape York in October. After leaving the latter place we have had no communication with any one but the folks at Port Essington, which is a mere military post, without any certain means of communication with England. We were ten weeks on our passage from Port Essington to Sydney and touched nowhere, so that you may imagine we were pretty well tired of the sea by the time we reached Port Jackson.

(vol. v.), in which, under the title of "Science at Sea," Huxley reviewed the *Voyage of the Rattlesnake* by MacGillivray, the naturalist to the expedition, which had recently appeared. This book gave very few descriptions of the incidents and life on board, and so drew in many ways a colourless picture of the expedition. This defect the reviewer sought to remedy by giving extracts from the so-called "unpublished correspondence" of one of the officers—sketches apparently written for the occasion—as well as from an equally unpublished but more real journal kept by the same hand.

The description of the ship herself, of her inadequate equipment for the special purposes she was to carry out, of the officers' quiet contempt of scientific pursuits, which not even the captain's influence was able to subdue, of the illusory promises of help and advancement held out by the Admiralty to young investigators, makes a striking foil to the spirit in which the Government of thirty years later undertook a greater scientific expedition. Perhaps some vivid recollections of this voyage did something to better the conditions under which the later investigators worked.

Thus, p. 100:

In the year 1846, Captain Owen Stanley, a young and zealous officer, of good report for his capabilities as a scientific surveyor, was entrusted with the command of the *Rattlesnake*, a vessel of six-and-twenty guns, strong and seaworthy, but one of that class unenviably distinguished in the war-time as a "donkey-frigate." To the laity it would seem that a ship journeying to unknown regions, when the lives of a couple of hundred men may, at any moment, depend upon her handiness in going about, so as to avoid any suddenly discovered danger, should possess the best possible sailing powers. The Admiralty, however, makes its selection upon other principles, and exploring vessels will be invariably found to be the slowest, clumsiest, and in every respect the most inconvenient ships which wear the pennant. In accordance with the rule, such was the *Rattlesnake*; and to carry out the spirit of the authorities more completely, she was turned out of Portsmouth dockyard in such a disgraceful state of unfitness, that her lower deck was continually under water during the voyage.

(vol. v.), in which, under the title of "Science at Sea," Huxley reviewed the *Voyage of the Rattlesnake* by MacGillivray, the naturalist to the expedition, which had recently appeared. This book gave very few descriptions of the incidents and life on board, and so drew in many ways a colourless picture of the expedition. This defect the reviewer sought to remedy by giving extracts from the so-called "unpublished correspondence" of one of the officers—sketches apparently written for the occasion—as well as from an equally unpublished but more real journal kept by the same hand.

The description of the ship herself, of her inadequate equipment for the special purposes she was to carry out, of the officers' quiet contempt of scientific pursuits, which not even the captain's influence was able to subdue, of the illusory promises of help and advancement held out by the Admiralty to young investigators, makes a striking foil to the spirit in which the Government of thirty years later undertook a greater scientific expedition. Perhaps some vivid recollections of this voyage did something to better the conditions under which the later investigators worked.

Thus, p. 100:

In the year 1846, Captain Owen Stanley, a young and zealous officer, of good report for his capabilities as a scientific surveyor, was entrusted with the command of the *Rattlesnake*, a vessel of six-and-twenty guns, strong and seaworthy, but one of that class unenviably distinguished in the war-time as a "donkey-frigate." To the laity it would seem that a ship journeying to unknown regions, when the lives of a couple of hundred men may, at any moment, depend upon her handiness in going about, so as to avoid any suddenly discovered danger, should possess the best possible sailing powers. The Admiralty, however, makes its selection upon other principles, and exploring vessels will be invariably found to be the slowest, clumsiest, and in every respect the most inconvenient ships which wear the pennant. In accordance with the rule, such was the *Rattlesnake*; and to carry out the spirit of the authorities more completely, she was turned out of Portsmouth dockyard in such a disgraceful state of unfitness, that her lower deck was continually under water during the voyage.

be patient, and listen to what I have to say; you will then, perhaps, be a little more content with your lot in life, and a little less desirous of mine. Of all extant lives, that on board a ship-of-war is the most artificial—whether necessarily so or not is a question I will not undertake to decide; but the fact is indubitable.

How utterly disgusted you get with one another! Little peculiarities which would give a certain charm and variety to social intercourse under any other circumstances, become sources of absolute pain, and almost uncontrollable irritation, when you are shut up with them day and night. One good friend and messmate of mine has a peculiar laugh, whose iteration on our last cruise nearly drove me insane.

There is no being alone in a ship. Sailors are essentially gregarious animals, and don't at all understand the necessity under which many people labour—I among the rest—of having a little solitary converse with oneself occasionally.

Then, to a landsman fresh from ordinary society and its peculiarly undemonstrative ways, there is something very wonderful about naval discipline. I do not mean to say that the subordination kept up is more than is necessary, nor perhaps is it in reality greater than is to be found in a college, or a regiment, or a large mercantile house; but it is made so *very* obvious. You not only feel the bit, but you see it; and your bridle is hung with bells to tell you of its presence.

Your captain is a very different person, in relation to his officers, from the colonel of a regiment; he is a demi-god, a Dalai lama, living in solitary state; sublime, unapproachable; and the radiation of his dignity stretches through all the other members of the nautical hierarchy; hence all sorts of petty intrigues, disputes, grumblings, and jealousies, which, to the irreverent eye of an "idler," give to the whole little society the aspect of nothing so much as the court of Prinz Irenæus in Kater Murr's inestimable autobiography.

P. 107 sq.:

After describing the illusory promises of the Admiralty and their grudging spirit towards the scientific members of the expedition, he continues:—

These are the *facilities and encouragement* to science afforded by the Admiralty; and it cannot be wondered at if the same spirit runs through its subordinate officers.

It is no exaggeration to say *hot*, for the temperature is that at which people at home commonly take a hot bath. It rains so hard that we have caught seven tons of water in one day, and it is therefore impossible to go on deck, though, if one did, one's condition would not be much improved. A *hot* Scotch mist covers the sea and hides the land, so that no surveying can be done; moving about in the slightest degree causes a flood of perspiration to pour out; all energy is completely gone, and if I could help it I would not think even; it's too hot. The rain awnings are spread, and we can have no wind sails up; if we could, there is not a breath of wind to fill them; and consequently the lower and main decks are utterly unventilated: a sort of solution of man in steam fills them from end to end, and surrounds the lights with a lurid halo. It's too hot to sleep, and my sole amusement consists in watching the cockroaches, which are in a state of intense excitement and happiness. They manifest these feelings in a very remarkable manner—a sudden unanimous impulse seems to seize the obscene thousands which usually lurk hidden in the corners of my cabin. Out they rush, helter-skelter, and run over me, my table, and my desk; others, more vigorous, fly, quite regardless of consequences, until they hit against something, upon which, half spreading their wings, they make their heads a pivot and spin round in a circle, in a manner which indicates a temporary aberration of the cockroach mind. It is these outbreaks alone which rouse us from our lassitude. Knocks are heard resounding on all sides, and each inhabitant of a cabin, armed with a slipper, is seen taking ample revenge upon the disturbers of his rest and the destroyers of his body and clothes.”

Here, on the other hand, is an oasis, a bartering scene at Bruny Island, in the *Louisiade*:—

“We landed at the same place as before, and this time the natives ran down prancing and gesticulating. Many of them had garlands of green leaves round their heads, knees, and ankles; some wore long streamers depending from their arms and ears and floating in the wind as they galloped along, shaking their spears and prancing just as boys do when playing at horses. They soon surrounded us, shouting ‘Kelumai! Kelumai!’ (their word for iron), and offering us all sorts of things in exchange. One very fine athletic man, ‘Kai-oo-why-who-at’ by name, was perfectly mad to get an axe, and very soon comprehended the arrangements that were made. Mr. Brady drew ten lines on the sand and laid an axe down by them,

we ingeniously coloured ours white, and were astonished to see that we were really of that (to them) disgusting tint all over."

On May 2, 1850, the *Rattlesnake* sailed for the last time out of Sydney harbour, bound for England by way of the Horn. In spite of his cheerful anticipations, Huxley was not to see his future wife again for five years more, when he was at length in a position to bid her come and join him. During the three years of their engagement in Australia, they had at least been able to see each other at intervals, and to be together for months at a time. In the long periods of absence, also, they had invented a device to cheat the sense of separation. Each kept a particular journal, to be exchanged when they met again, and only to be read, day by day, during the next voyage. But now it was very different, their only means of communication being the slow agency of the post, beset with endless possibilities of misunderstanding when it brought belated answers to questions already months old and out of date in the changed aspect of circumstances. These perils, however, they weathered, and it proves how deep in the moral nature of each the bond between them was rooted, that in the end they passed safely through the still greater danger of imperceptibly growing estranged from one another under the influences of such utterly different surroundings.

A kindly storm which forced the old ship to put into the Bay of Islands to repair a number of small leaks that rendered the lower deck uninhabitable, made it possible for Huxley to send back a letter that should reach Australia in one month instead of ten after his departure.

He utilized a week's stay here characteristically enough in an expedition to Waimate, the chief missionary station and the school of the native institutions (a sort of Normal School for native teachers), in order to judge of his own inspection what missionary life was like.

I have been greatly surprised in these good people (he writes). I had expected a good deal of *straight-hairedness* (if you understand the phrase) and methodistical puritanism, but I find it quite otherwise. Both Mr. and Mrs. Burrows seem

think I shall go and look them up under pretence of making a call. They say that the present winter is far more savage than the generality of Falkland Island winters, and it had need be, for I never felt anything so bitterly cold in my life. The thermometer has been down below 22, and shallow parts of the harbour even have frozen. Nothing to be done ashore. My rifle lies idle in its case; no chance of a shot at a bull, and one has to go away 20 miles to get hold even of the upland geese and rabbits. The only thing to be done is to eat, eat, eat, and the cold assists one wonderfully in that operation. You consume a pound or so of beefsteaks at breakfast and then walk the deck for an appetite at dinner, when you take another pound or two of beef or a goose, or some such trifle. By four o'clock it is dark night, and as it is too cold to read the only thing to be done is to vanish under blankets as soon as possible and take twelve or fourteen hours' sleep.

Mrs. Stanley's Bougirigards,* which I have taken under my care during the cold weather, admire this sort of thing exceedingly and thrive under it, so I suppose I ought to.

The journey from New Zealand here has been upon the whole favourable; no gales—quite the reverse—but light variable winds and calms. The latter part of our voyage has, however, been very cold, snow falling in abundance, and the ice forming great stalactites about our bows. We have seen no icebergs nor anything remarkable. From all I can learn it is most probable that we shall leave in about a week and shall go direct to England without stopping at any other port. I wish it may be so. I want to get home and look about me.

We have had news up to the end of March. There is nothing of any importance going on. By the Navy list for April I see that I shall be as nearly as possible in the middle of those of my own rank, *i.e.* I shall have about 150 above and as many below me. This is about what I ought to expect in the ordinary run of promotion in eight years, and I have served four and a half of that time. I don't expect much in the way of promotion, especially in these economic times; but I do not fear that I shall be able to keep me in England for at least a year after our arrival, in order to publish my papers. The Admiralty have quite recently published a distinct declaration that they will consider scientific attainments as a claim to their notice, and I expect to be the first to remind them of their promise, and

* The Australian love-bird; a small parrakeet.

I will take care to have the reminder so backed that they must and shall take note of it. Even if they will not promote me at once, it would answer our purpose to have an appointment to some ship on the home station for a short time.

The last of the Falklands was seen on July 25; the line was crossed in thirty-six days; another month, and water running short, it was found necessary to put in at the Azores for a week. Leaving Fayal on October 5, the *Rattlesnake* reached Plymouth on the 23rd, but next day proceeded to Chatham, which, thanks to baffling winds, was not reached till November 9, when the ship was paid off.

CHAPTER V

1850-1851

IN the Huxley Lecture for 1898 (*Times*, October 4) Professor Virchow takes occasion to speak of the effect of Huxley's service in the *Rattlesnake* upon his intellectual development:—

When Huxley himself left Charing Cross Hospital in 1846, he had enjoyed a rich measure of instruction in anatomy and physiology. Thus trained, he took the post of naval surgeon, and by the time that he returned, four years later, he had become a perfect zoologist and a keen-sighted ethnologist. How this was possible any one will readily understand who knows from his own experience how great the value of personal observation is for the development of independent and unprejudiced thought. For a young man who, besides collecting a rich treasure of positive knowledge, has practised dissection and the exercise of a critical judgment, a long sea-voyage and a peaceful sojourn among entirely new surroundings afford an invaluable opportunity for original work and deep reflection. Freed from the formalism of the schools, thrown upon the use of his own intellect, compelled to test each single object as regards properties and history, he soon forgets the dogmas of the prevailing system and becomes, first a sceptic, and then an investigator. This change, which did not fail to affect Huxley, and through which arose that Huxley whom we commemorate to-day, is no unknown occurrence to one who is acquainted with the history, not only of knowledge, but also of scholars.

But he was not destined to find his subsequent path easy. Once in England, indeed, he did not lose any time. No sooner had the *Rattlesnake* touched at Plymouth than

In this design he was fortified by his old Haslar friend, Dr. (afterwards Sir John) Watt Reid, who wrote: "They cannot, and, I am sure, will not wish to stand in your way at Whitehall." Meanwhile, the first person, naturally, he had thought of consulting was his old chief, Sir John Richardson, who had great weight at the Admiralty, and to him he wrote the following letter before leaving Plymouth.

TO SIR JOHN RICHARDSON

Oct. 31, 1850.

I regret very much that in consequence of our being ordered to be paid off at Chatham, instead of Portsmouth, as we always hoped and expected, I shall be unable to submit to your inspection the zoological notes and drawings which I have made during our cruise. They are somewhat numerous (over 180 sheets of drawings), and I hope not altogether valueless, since they have been made with as great care and attention as I am master of—and with a microscope, such as has rarely, if ever, made a voyage round the world before. A further reason for indulging in this hope consists in the fact that they relate for the most part to animals hitherto very little known, whether from their rarity or from their perishable nature, and that they bear upon many curious physiological points.

I may thus classify and enumerate the observations I have made—

1. Upon the organs of hearing and circulation in some of the transparent Crustacea, and upon the structure of certain of the lower forms of Crustacea.

2. Upon some very remarkable new forms of Annelids, and especially upon the much contested genus *Sagitta*, which I have evidence to show is neither a Mollusc nor an Epizoon, but an Annelid.

3. Upon the nervous system of certain Mollusca hitherto imperfectly described—upon what appears to me to be an urinary organ in many of them—and upon the structure of *Firola* and *Atlanta*, of which latter I have a pretty complete account.

4. Upon two perfectly new (ordinally new) species of Ascidians.

5. Upon *Pyrosoma* and *Salpa*. The former has never been described (I think) since Savigny's time, and he had only specimens preserved in spirits. I have a great deal to add and alter.

(1) of procuring a grant for the publication of my papers, or (2) should that not be feasible, to obtain a nominal appointment (say to the *Fisguard* at Woolwich, as in Dr. Hooker's case) for such time as might be requisite for the publication of my papers and drawings in some other way?

I shall see Professors Owen and Forbes when I reach London, and I have a letter of introduction to Sir John Herschel (who has, I hear, a great penchant for the towing-net). Supposing I could do so, would it be of any use to procure recommendations from them that my papers should be published?

[(Half-erased) To Sir F. Beaufort also I have a letter.] Would it not be proper also to write to Sir W. Burnett acquainting him with my views, and requesting his acquiescence and assistance?

Begging an answer at your earliest convenience, addressed either to the *Rattlesnake* or to my brother, I remain, your obedient servant,

T. H. HUXLEY.

41 North Bank.

He received a most friendly reply from "Old John." He was willing to do all in his power to help, but could recommend Government aid better if he had seen the drawings. Meantime a certificate should be got from Forbes, the best man in this particular branch of science, backed, if possible, by Owen. He would speak to some officials himself, and give Huxley introductions to others, and if he could get up to town, would try to see the collections and add his name to the certificate.

Both Forbes and Owen were ready to help. The former wrote a most encouraging letter, singling out the characteristics which gave a peculiar value to these papers:—

I have had very great pleasure in examining your drawings of animals observed during the voyage of the *Rattlesnake*, and have also fully availed myself of the opportunity of going over the collections made during the course of the survey upon which you have been engaged. I can say without exaggeration that more important or more complete zoological researches have never been conducted during any voyage of discovery in the southern hemisphere. The course you have taken of directing your attention mainly to impreservable creatures, and to those orders of the animal kingdom respecting which we have least

Properly speaking, indeed, we have been at home longer, for we touched at Plymouth and trod English ground and saw English green fields on the 23rd of October, but we were allowed to remain only twenty-four hours, and to my great disgust were ordered round to Chatham to be paid off. The ill-luck which had made our voyage homeward so long (we sailed from Sydney on the 2nd of May) pursued us in the Channel, and we did not reach Chatham until the 2nd of November; and what do you think was one of the first things I did when we reached Plymouth? Wrote to Eliza K. asking news of a certain naughty sister of mine, from whom I had never heard a word since we had been away—and if perchance there should be any letter, begging her to forward it immediately to Chatham. And so, when at length we got there, I found your kind long letter had been in England some six or seven months; but hearing of the likelihood of our return, they had very judiciously not sent it to me.

Your letter, my poor Lizzie, justifies many a heartache I have had when thinking over your lot, knowing, as I well do, what emigrant life is in climates less trying than that in which you live. I have seen a good deal of bush life in Australia, and it enables me fully to sympathise with and enter into every particular you tell me—from the baking and boiling and pigs squealing, down to that ferocious landshark Mrs. Gunther, of whose class Australia will furnish fine specimens. Had I been at home, too, I could have enlightened the good folks as to the means of carriage in the colonies, and could have told them that the two or twenty thousand miles over sea is the smallest part of the difficulty and expense of getting anything to people living inland; as it is, I think I have done some good in the matter; their meaning was good but their discretion small. But the obtuseness of English in general about anything out of the immediate circle of their own experience is something wonderful.

I had heard here and there fractional accounts of your doings from Eliza K. and my mother—not of the most cheery description—and therefore I was right glad to get your letter, which, though it tells of sorrow and misfortune enough and to spare, yet shows me that the brave woman's heart you always had, my dearest Lizzie, is still yours, and that you have always had the warm love of those immediately around you, and now, as the doctor's letter tells us, you have one more source of joy and happiness, and this new joy must efface the bitterness—

do. It would be glorious to be a voice working in secret and free from all those personal motives that have actuated the best. But, unfortunately, one is not a "vox et præterea nihil," but with a considerable corporality attached which requires feeding, and so while my inner man is continually indulging in these anchorite reflections, the outer is sedulously elbowing and pushing as if he dreamed of nothing but gold medals and professors' caps.

I am getting on very well—better I fear than I deserve. One of my papers was published in 1849 in the *Philosophical Transactions*, another in the *Zoological Transactions*, and some more may be published in the *Linnæan* if I like—but I think I shall not like. Then I have worked pretty hard, and brought home a considerable amount of drawings and notes about new or rare animals, all particularly nasty slimy things, and they will most likely be published as a separate work by the Royal Society.

Owens, Forbes, Bell, and Sharpey (the doctor will tell you of what weight these names are) are all members of the committee which disposes of the money, and are all strongly in favour of my "valuable researches" (cock-a-doodle-doo!!) being published by the Society. From various circumstances I have taken a better position than I could have expected among these grandees, and I find them all immensely civil and ready to help me on, tooth and nail, particularly Prof. Forbes, who is a right good fellow, and has taken a great deal of trouble on my behalf. Owen volunteered to write to the "First Lord" on my behalf, and did so. Sharpey, when I saw him, reminded me, as he always does, of my great contest with Stocks* (do you remember throwing the shoe?), and promised me all the assistance in his power. Prof. Bell, who is secretary to the Royal, and has great influence, promised to help me in every way, and asked me to dine with him and meet a lot of nobs. I take all these things quite as a matter of course, but am all the while considerably astonished. The other day I dined at the Geological Club and met Lyell, Murchison, de la B[eche] Horner, and a lot more, and last evening I dined with a whole lot of literary and scientific people.

Owen was, in my estimation, great, from the fact of his smoking his cigar and singing his song like a brick.

I tell you all these things to show you clearly how I stand.

*.See p. 19.

could not get her the bird with the long tail, but that some day I will send her some pictures of copper-coloured gentlemen with great big wigs and no trousers, and tell her her old uncle loves her very much and never forgets her nor anybody else.

God bless you, dearest Lizzie. Write soon. — Ever your brother,
Tom.

Thus within a month of landing in England, Huxley had secured his footing in the scientific world. He was freed for the time from the more irksome part of his profession; his service in the navy had become a stepping-stone to the pursuits in which his heart really was. He had long been half in despair over the work which he had sent out like the dove from the ark, if haply it might find him some standing ground in the world; no news of it had reached him till he was about to start on his homeward voyage, but he returned to discover that at a single stroke it had placed him in the front rank of naturalists.

41 NORTH BANK, REGENT'S PARK,
Jan. 3, 1851.

My progress (he writes),* must necessarily be slow and uncertain. I cannot see two steps forwards. Much depends upon myself, much upon circumstances. Hitherto all has gone as well as I could wish. I have gained each object that I had set before myself—that is, I have my shore appointment, I have found a means of publishing what I have done creditably, and I have continued to come into communication with some of the first men in England in my department of science. But, as I have found to be the case in all things that are gained, from money to friendship, it is not so much getting as keeping. It is by no means difficult if you are decently introduced, have tolerably agreeable manners, and some smattering of science, to take a position among these folks, but it is a mighty different affair to keep it and turn it to account. Not like the man who, at the Enchanted Castle, had the courage to blow the horn but not to draw the sword, and was consequently shot forth from the mouth of the cave by which he entered with most ignominious haste,—one must be ready to fight immediately after one's arrival has been announced, or be blown into oblivion.

* When not otherwise specified, the extracts in this chapter are from letters to his future wife.

him the greatest kindness throughout this period of struggle, and the sympathy and intellectual stimulus he received from their society were of the utmost help. They were always ready to welcome him at Greenwich, and he not only often ran down there for a week-end, but would spend part of his vacations with them at Lowestoft or Tenby, where naturalists could find plenty of occupation.

But from a worldly point of view, it was too soon clear that science was sadly unprofitable. There seemed no speedy prospect of making enough to marry on. As early as March 1851 he writes:—

The difficulties of obtaining a decent position in England in anything like a reasonable time seem to me greater than ever they were. To attempt to live by any scientific pursuit is a farce. Nothing but what is absolutely practical will go down in England. A man of science may earn great distinction, but not bread. He will get invitations to all sorts of dinners and conversaziones, but not enough income to pay his cab fare. A man of science in these times is like an Esau who sells his birth-right for a mess of pottage. Again, if one turns to practice, it is still the old story—wait; and only after years of working like a galley-slave and intriguing like a courtier is there any chance of getting a decent livelihood. I am not at all sure if . . . it would not be the most prudent thing to stick by the Service: there at any rate is certainty in health and in sickness.

Nevertheless he was mightily encouraged in the work of bringing out his *Rattlesnake* papers by a notable success in a quarter where he scarcely dared to hope for it. The Royal Society had for some time set itself to become a body of working men of science; to exclude for the future all mere dilettanti, and to admit a limited number of men whose work was such as to deserve recognition. Thanks to the initiative of Forbes, he now found this recognition accorded to him on the strength of his “Medusa” paper. He writes in February:—

The F.R.S. that you tell me you dream of being appended to my name is nearer than one might think, to my no small surprise. . . . I had no idea that it was at all within my reach, until I found out the other day, talking with Mr. Bell, that my having a paper in the *Transactions* was one of the best of qualifications.

Again, on May 4:—

I am twenty-six to-day . . . and it reminds me that I have left you now a whole year. It is perfectly frightful to think how the time is slipping by, and yet seems to bring us no nearer.

What have I done with my twenty-sixth year? Six months were spent at sea, and therefore may be considered as so much lost; and six months I have had in England. That, I may say, has not been thrown away altogether without fruit. I have read a good deal and I have written a good deal. I have made some valuable friends, and have found my work more highly estimated than I had ventured to hope. I must tell you something, because it will please you, even if you think me vain for doing so.

I was talking to Professor Owen yesterday, and said that I imagined I had to thank him in great measure for the honour of the F.R.S. "No," he said, "you have nothing to thank but the goodness of your own work." For about ten minutes I felt rather proud of that speech, and shall keep it by me whenever I feel inclined to think myself a fool, and that I have a most mistaken notion of my own capacities. The only use of honours is as an antidote to such fits of the "blue devils." Of one thing, however, which is by no means so agreeable, my opportunities for seeing the scientific world in England force upon me every day a stronger and stronger conviction. It is that there is no chance of living by science. I have been loth to believe it, but it is so. There are not more than four or five offices in London which a Zoologist or Comparative Anatomist can hold and live by. Owen, who has a European reputation, second only to that of Cuvier, gets as Hunterian Professor £300 a year! which is less than the salary of many a bank clerk. My friend Forbes, who is a highly distinguished and a very able man, gets the same from his office of Paleontologist to the Geological Survey of Great Britain. Now, these are first-rate men—men who have been at work for years laboriously toiling upward—men whose abilities, had they turned them into the many channels of money-making, must have made large fortunes. But the beauty of Nature and the pursuit of Truth allured them into a nobler life—and this is the result. . . . In literature a man may write for magazines and reviews, and so support himself; but not so in science. I could get anything I write into any of the journals or any of the Transactions, but I know no means of thereby earning five shillings. A man who chooses a life of science

Do you understand this? I know you do; our old oneness of feeling will not desert us here. . . .

To-day a most unexpected occurrence came to my knowledge. I must tell you that the Queen places at the disposal of the Royal Society once a year a valuable gold medal to be given to the author of the best paper upon either a physical, chemical, or anatomical or physiological subject. One of these branches of science is chosen by the Royal Society for each year, and therefore for any given subject—say anatomy and physiology; it becomes a triennial prize, and is given to the best memoir in the *Transactions* for three years.

It happens that the Royal Medal, as it is called, is this year given in Anatomy and Physiology. I had no idea that I had the least chance of getting it, and made no effort to do so. But I heard this morning from a member of the Council that the award was made yesterday, and that I was within an ace of getting it. Newport, a man of high standing in the scientific world, and myself were the two between whom the choice rested, and eventually it was given to him, on account of his having a greater bulk of matter in his papers, so evenly did the balance swing. Had I only had the least idea that I should be selected they should have had enough and to spare from me. However, I do not grudge Newport his medal; he is a good and a worthy competitor, old enough to be my father, and has long had a high reputation. Except for its practical value as a means of getting a position I care little enough for the medal. What I do care for is the justification which the being marked in this position gives to the course I have taken. Obstinate and self-willed as I am . . . there are times when grave doubts overshadow my mind, and then such testimony as this restores my self-confidence.

To let you know the full force of what I have been saying, I must tell you that this "Royal Medal" is what such men as Owen and Faraday are glad to get, and is indeed one of the highest honours in England.

To-day I had the great pleasure of meeting my old friend Sir John Richardson (to whom I was mainly indebted for my appointment in the *Rattlesnake*). Since I left England he has married a third wife, and has taken a hand in joining in search of Franklin (which was more dreadful?), like an old hero as he is; but not a feather of him is altered, and he is as grey, as really kind, and as seemingly abrupt and grim, as ever he was. Such a fine old polar bear!

CHAPTER VI

1851-1854

THE course pursued by the Government in the matter of Huxley's papers is curious and instructive. The Admiralty minute of 1849 had promised either money assistance for publishing or speedy promotion as an encouragement to scientific research in the Navy, especially by the medical officers. On leave to publish the scientific results of the expedition being asked for, the Department forestalled any request for monetary aid by an intimation that none would be given. Strong representations, however, from the leading scientific authorities induced them to grant the appointment to the *Fisguard* for six months.

The sequel shows how the departmental representatives of science did their best for science in Huxley's case, so far as in their power lay:—

June 6, 1851.—The other day I received an intimation that my presence was required at Somerset House. I rather expected the mandate, as six months' leave was up. Sir William was very civil, and told me that the Commander of the *Fisguard* had applied to the Admiralty to know what was to be done with me, as my leave had expired. "Now," said he, "go to Forrest" (his secretary), "write a letter to me, stating what you want, and I will get it done for you." So away I went and applied for an indefinite amount of leave, on condition of reporting the progress of my work every six months, and as I suppose I shall get it, I feel quite easy on that head.

In May 1851 he applied to the Royal Society for help from the Government Grant towards publishing the bulk of his work as a whole, for much of its value would be lost

if scattered fragmentarily among the Transactions of various learned societies. Personally, the members of the committee were very willing to make the grant, but on further consideration it appeared that the money was to be applied for promoting research, not for assisting publication; and moreover, it was desirable not to establish a precedent for saddling the funds at the disposal of the Society with all the publications which it was the clear duty of the Government to undertake. On this ground the application was refused, but at the same time it was resolved that the Government be formally asked to give the necessary subvention towards bringing out these valuable papers.

A similar resolution was passed at the Ipswich meeting of the British Association in July 1851, and at a meeting of its Council in March 1852 the President declared himself ready to carry it into effect by asking the Treasury for the needful £300. But at the July meeting he could only report a *non possumus* answer for the current year (1852) from the Government, and a resolution was passed recommending that application on the subject be renewed by the British Association in the following year.

Meanwhile, weary of official delay, Huxley had conceived the idea of writing direct to the Duke of Northumberland, then First Lord of the Admiralty, whom he knew to take an interest in scientific research. At the same time he stirred Lord Rosse, the President of the Royal Society, to repeat his application to the Treasury. Although the Admiralty in April 1852 again refused money help, and bade him apply to the Royal Society for a portion of the Government Grant (which the latter had already refused him), the Hydrographer was directed to make inquiries as to the propriety of granting him an extension of leave. To his question asking the exact amount of time still required for finishing the work of publication, Huxley returned what he described as a "savage reply," that his experience of engravers led him to think that the plates could be published in eight or nine months from the receipt of a grant; that he had reason to believe this grant might soon be promised, but that the long delay was solely due to the remissness

of those whose duty it was to represent his claims to the Government; and finally, that he must ask for a year's extension of leave.

For these expressions his conscience smote him when, on June 12, at a soirée of the Royal Society, Lord Rosse took him aside and informed him that he had seen Sir C. Trevelyan, the Under Secretary to the Treasury, who said there would be no difficulty in the matter if it were properly laid before the Prime Minister, Lord Derby. To Lord Derby therefore he went, and was told that Mr. Huxley should go to the Treasury and arrange matters in person with Trevelyan. At the same time the indignant tone of his letter to the Hydrographer seemed to have done good; he was invited to explain matters in person, and was granted the leave he asked for.

Everything now seemed to point to a speedy solution of his difficulties. The promise of a grant, of course, did nothing immediate, but assured him a good position, and settled all the scruples of the Admiralty with regard to time. "You have no notion," he writes, "of the trouble the grant has cost me. It died a natural death till I wrote to the Duke in March, and brought it to life again. The more opposition there is, the more determined I am to carry it through." But he was doomed to a worse disappointment than before. Trevelyan received him very civilly, but had heard nothing on the matter from Lord Derby, and accordingly sent him in charge of his private secretary to see Lord Derby's secretary. The latter had seen no papers relating to any such matter, and supposed Lord Derby had not brought them from St. James' Square, "but promised to write to me as soon as anything was learnt. I look upon it as adjourned *sine die*." Parliament breaking up immediately after gave the officials a good excuse for doing nothing more.

When his year's leave expired in June 1853, he wrote the following letter to Sir William Burnett:—

As the period of my leave of absence from H.M.S. *Fisguard* is about to expire, I have the honour to report that the duty on which I have been engaged has been carried out, as far as my

means permit, by the publication of a "Memoir upon the Homologies of the Cephalous Mollusca," with four plates, which appeared in the *Philosophical Transactions* for 1852 (published 1853), being the fourth memoir resulting from the observations made during the voyage of H.M.S. *Rattlesnake* which has appeared in these *Transactions*.

I have the pleasure of being able to add that the President and Council of the Royal Society have considered these memoirs worthy of being rewarded by the Royal Medal in Physiology for 1852, which they did me the honour to confer in the November of that year.

I regret that no definite answer of any kind having as yet been given to the strong representations which were made by the Presidents both of the Royal Society and of the British Association in 1852 to H.M. Government—representations which have recently been earnestly repeated—in order to obtain a grant for the purpose of publishing the remainder of these researches in a separate form, I have been unable to proceed any further, and I beg to request a renewal of my leave of absence from H.M.S. *Fisguard*, so that if H.M. Government think fit to give the grant applied for, it may be in my power to make use of it; or that, should it be denied, I may be enabled to find some other means of preventing the total loss of the labour of some years.

Hereupon he was allowed six months longer, but with the intimation that no further leave would be granted. A final application from the scientific authorities resulted in fresh inquiries as to the length of time still required, and the deadlock between the two departments of State being unchanged, he replied to the same effect as before, but to no purpose. His formal application for leave in January 1854 was met by orders to join the *Illustrious* at Portsmouth. He appealed to the Admiralty that this appointment might be cancelled, giving a brief summary of the facts, and pointing out that it was the inaction of the Treasury which had absolutely prevented him from completing his work.

I would therefore respectfully submit that, under these circumstances, my request to be permitted to remain on half-pay until the completion of the publication of the results of some years' toil is not wholly unreasonable. It is the only reward for which I would ask their Lordships, and indeed, considering the distinct pledge given in the minute to which I have referred,

3. Report upon the Development of the Echinoderms. To appear in the *Annals* for July.

4. On the Anatomy and Physiology of the Salpæ, with four plates. Read at the Royal Society, and to be published in the next part of the *Philosophical Transactions*.

5. On two Genera of Ascidians, *Doliolum* and *Appendicularia*, with one plate. Read at the Royal Society, and to be published in the next part of the *Philosophical Transactions*.

6. On some peculiarities in the Circulation of the Mollusca. Sent to M. Milne-Edwards, at his request, to be published in the *Annales des Sciences*.

7. On the Generative Organs of the Physophoridæ and Diphydæ. Sent to Prof. Müller of Berlin for publication in his *Archiv*.

By the end of the year he had four more to report:—
1. On the Hydrostatic Acalephæ; 2. On the genus *Sagitta*, both published in the *Report of the British Association* for 1851; 3. On *Lacinularia Socialis*, a contribution to the anatomy and physiology of the Rotifera, in the *Transactions of the Microscopical Society*; 4. On *Thalassicolla*, a new zoophyte, in the *Annals of Natural History*. Next year he read before the British Association a paper entitled “Researches into the Structure of the Ascidians,” and a very important one on the Morphology of the Cephalous Mollusca, afterwards published in the *Philosophical Transactions*. In addition he had prepared a great part of his longer work for publication; out of twenty-four or twenty-five plates, nineteen were ready for the engraver when he wrote his appeal to the Duke of Northumberland. In this same year, 1852, he was also awarded the Royal Medal in Physiology for the value of his contributions to the *Philosophical Transactions*.

In 1853, besides seeing some of these papers through the press, he published one on the existence of Cellulose in the Tunic of Ascidians, read before the Microscopical Society, and two papers on the Structure of the Teeth; the latter, of course, like a paper of the previous year on *Echinococcus*, being distinct from the *Rattlesnake* work. The greater work on Oceanic Hydrozoa, over which the battle

41 NORTH BANK, REGENT'S PARK,
May 7, 1852.

MY DEAR TYNDALL—Allow me to be one of the first to have the pleasure of congratulating you on your new honours. I had the satisfaction last night to hear your name read out as one of the selected of the Council of the Royal Society for election to the Fellowship this year, and you are therefore as good as elected.

I always made sure of your success, but I am not the less pleased that it is now a *fait accompli*.—I am, my dear Tyndall,
faithfully yours,
T. H. HUXLEY.

P.S.—I have heard nothing of Toronto, and I begin to think that the whole affair, University and all, is a myth.

His hopes of the Colonies failing, he tried each of the divisions of the United Kingdom in turn, with uniform ill-success; in 1852–53 at Aberdeen and at Cork; in 1853 at King's College, London. He had great hopes of Aberdeen at first; the appointment lay with the Home Secretary, a personal friend of Sir J. Clark, who was interested in Huxley though not personally acquainted with him. But no sooner had he written to urge the latter's claims than a change of ministry took place, and other influences commanded the field. It was cold comfort that Clark told him only to wait—something must turn up. There was still a great probability of the Toronto chair falling to a Cork professor; so with this in view, he gave up a trip to Chamonix with his brother, and attended the meeting of the British Association at Belfast in August 1852, in order to make himself known to the Irish men of science, for, as his friends told him, personal influence went for so much, and while most men's reputations were better than themselves, he might flatter himself that he was better than his reputation. But this, too, came to nothing, and the King's College appointment also went to the candidate who was backed by the most powerful influence.

A fatality seemed to dog his efforts; nevertheless he writes at the end of 1851:—

Among my scientific friends the monition I get on all sides is that of Dante's great ancestor to him—

Se tu segui la tua stella.

with no excitement and no occupation for the higher powers of the intellect, with its great aspirations stifled and all the great problems of existence set hopelessly in the background, offers to me a prospect that would be utterly intolerable but for your love. . . . Sometimes I am half mad with the notion of burying all my powers in a mere struggle for a livelihood. Sometimes I am equally wild at thinking of the long weary while that has passed since we met. There are times when I cannot bear to think of leaving my present pursuits, when I feel I should be guilty of a piece of cowardly desertion from my duty in doing it, and there come intervals when I would give truth and science and all hopes to be folded in your arms. . . . I know which course is right, but I never know which I may follow; help me . . . for there is only one course in which there is either hope or peace for me.

These repeated disappointments deepened the fits of depression which constantly assailed him. He was torn by two opposing thoughts. Was it just, was it right, to demand so great a sacrifice from the woman who had entrusted her future to the uncertain chances of his fortunes? Could he ask her to go on offering up the best years of her life to aspirations of his which were possibly chimerical, or perhaps merely selfishness in disguise, which ought to yield to more imperative duties? Why not clip the wings of Pegasus, and descend to the sober, everyday jog-trot after plain bread and cheese like other plain people? Time after time he almost made up his mind to throw science to the winds; to emigrate and establish a practice in Sydney; to try even squatting or storekeeping. And yet he knew only too well that with his temperament no life would bring him the remotest approach to lasting happiness and satisfaction except one that gave scope to his intellectual passion. To yield to the immediate pressure of circumstances was perhaps ignoble, was even more probably a surer road to the loss of happiness for himself and for his wife than the repeated and painful sacrifices of the present. With all this, however, and the more when assured of her entire confidence in his judgment, he could not but feel a sense of remorse that she willingly accepted the sacrifice, and feared that she might have done so rather to gratify his

read her thoughts as to whether he were right or wrong in the course he was pursuing. He appeals to her faith that he is choosing the nobler path in pursuing knowledge, than in turning aside to the temptation of throwing it up for the sake of their speedier union. Still she was right in claiming a share in his work; but for her his life would have been wasted.

The clouds gathered very thickly about him when in April 1852 his mother died, while his father was hopelessly ill. "Belief and happiness," he writes, "seem to be beyond the reach of thinking men in these days, but courage and silence are left." Again the clouds lifted, for in October he received Miss Heathorn's "noble and self-sacrificing letter, which has given me more comfort than anything for a long while," the keynote of which was that a man should pursue those things for which he is most fitted, let them be what they will. He now felt free to tell the vicissitudes of thought and will he had passed through this twelvemonth, and how the idea of giving up all had affected him. "The spectre of a wasted life has passed before me—a vision of that servant who hid his talent in a napkin and buried it."

Early in 1853 he writes how much he was cheered by his sister's advice and encouragement to persist in the struggle; but the darkest moment was still to come. His hopes from his candidature crumbled away one after the other; his leave from the Admiralty was coming to an end, and there was small hope of renewing it; the grant from Government remained as unattainable as ever; the long struggle had taught him the full extent of his powers only, it seemed, to end by denying him all opportunity for their use.

And so the card house I have been so laboriously building up these two years with all manner of hard struggling will be tumbled down again, and my small light will be ignominiously snuffed out like that of better men. . . . I can submit if the fates are too strong. The world is no better than an arena of gladiators, and I, a stray savage, have been turned into it to fight my way with my rude club among the steel-clad fighters. Well, I have won my way into the front rank, and ought to be thankful

. . . It is as if all that old life at Holmwood had merely been a preparation for the real life of our love—as if we were then children ignorant of life's real purpose—as if these last months had merely been my old doubts over again, whether I had rightly or wrongly interpreted the manner and the words that had given me hope. . . .

We will begin the new love of woman and man, no longer that of boy and girl, conscious that we have aims and purposes as well as affections, and that if love is sweet life is dreadfully stern and earnest.

As time went on and no permanency offered—although a good deal of writing fell in his way—the strain told heavily upon him. In the autumn he was quite out of sorts, body and mind, more at war with himself than he ever was in his life before. All this, he writes, had darkened his thoughts, had made him once more imagine a hopeless discrepancy between the two of them in their ways of thinking and objects in life. It was not till November 1853 that this depression was banished by the trust and confidence of her last letter. “I wish to Heaven,” he writes, “it had reached me six months ago. It would have saved me a world of pain and error.” But with this, the worst period of mental suffering was over, and every haunting doubt was finally exorcised. His career was made possible by the steady faith which neither separation nor any misgiving nor its own troubles could shake. And from this point all things began to brighten. His health had been restored by a trip to the Pyrenees with his brother George in September. He had got work that enabled him to regard the Admiralty and its menaces with complete equanimity; a *Manual of Comparative Anatomy*, for Churchill the publisher, regular work on the *Westminster*,* and another book in prospect, “so that if I quit the Service to-morrow, these will give me more than

* This regular work was the article on Contemporary Science, which in October 1854 he got Tyndall to share with him. For, he writes, “To give some account of the books in one's own department is no particular trouble, and comes with me under the head of being paid for what I *must*, in any case, do—but I neither will, nor can, go on writing about books in other departments, of which I am not competent to form a judgment even if I had the time to give to them.”

my pay has been." And on December 7 he writes how he has been restored and revived by reading over her last two letters, and confesses, "I have been unjust to the depth and strength of your devotion, but will never do so again." Then he tells all he had gone through before leaving England in September for his holiday—how he had resolved to abandon all his special pursuits and take up Chemistry, for practical purposes, when first one publisher and then another asked him to write for them, and hopes were held out to him of being appointed to deliver the Fullerian lectures at the Royal Institution for the next three years; while, most important of all, Edward Forbes was likely before long, to leave his post at the Museum of Practical Geology, and he had already been spoken to by the authorities about filling it. This was worth some £200 a year, while he calculated to make about £250 by his pen alone. "Therefore it would be absurd to go hunting for chemical birds in the bush when I have such in the hand."

CHAPTER VII

1851-1853

SEVERAL letters dating from 1851 to 1853 help to fill up the outlines of Huxley's life during those three years of struggle. There is a description of the British Association meeting at Ipswich in 1851,* with the traditional touch of gaiety to enliven the gravity of its proceedings, and the unconventional jollity of the Red Lion Club (a dining-club of members of the Association), whose palmy days were those under the inspiration of the genial and gifted Forbes. This was the meeting at which Huxley first began his alliance with Tyndall, with whom he travelled down from town, although he does not mention his name in this letter. With Hooker he had already made acquaintance; and from this time forwards the three were closely bound together by personal regard as well as by similarity of aims and interests.

Then follow his sketch of the English scientific world as he found it in 1851, given in his letter to W. Macleay; several letters to his sister; the description of his first lecture at the Royal Institution, which, though successful on the whole, was very different in manner and delivery from the clear and even flow of his later style, with the voice not loud but distinct, the utterance never hurried beyond the point of immediate comprehension, but carrying the attention of the audience with it, eager to the end. Two letters of warning and remonstrance against the habits of lecturing

* "Forbes advises me to go down to the meeting of the British Association this year and make myself notorious somehow or other. Thank Heaven I have impudence enough to lecture the savans of Europe if necessary. Can you imagine me holding forth?" (June 6, 1851.)

July 16, 1851.—I went yesterday to dine with Colonel Sabine. We had a long discourse about the prospects and probable means of existence of young men trying to make their way to an existence in the scientific world. I took, as indeed what I have seen has forced me to take, rather the despairing side of the question, and said that as it seemed to me England did not afford even the means of existence to young men who were willing to devote themselves to science. However, he spoke cheerfully, and advised me by no means to be hasty, but to wait, and he doubted not that I should succeed. He cited his own case as an instance of waiting, eventually successful. Altogether I felt the better for what he said. . . .

There has been a notice of me in the *Literary Gazette* for last week, much more laudatory than I deserve, from the pen of my friend Forbes.* . . .

In the same number is a rich song from the same fertile and versatile pen, which was sung at one of our Red Lion meetings. That is why I want you to look at it, not that you will understand it, because it is full of allusions to occurrences known only in the scientific circles. At Ipswich we had a grand Red Lion meeting; about forty members were present, and among them some of the most distinguished members of the Association. Some foreigners were invited (the Prince of Canino, Buonaparte's nephew, among others), and were not a little astonished to see the grave professors, whose English solemnity and gravity they had doubtless commented on elsewhere, giving themselves up to all sorts of fun. Among the Red Lions we have a custom (instead of cheering) of waving and wagging one coat-tail (one Lion's tail) when we applaud. This seemed to strike the Prince's fancy amazingly, and when he got up to return thanks for his health being drunk, he told us that as he was rather out of practice in speaking English, he would return thanks in our fashion, and therewith he gave three mighty roars and wags, to the no small amusement of every one. He is singularly like the portraits of his uncle, and seems a very jolly, good-humoured old fellow. I believe, however, he is a bit of a rip. It was remarkable how proud the Quakers were of being noticed by him.

* An appreciation of his papers on the Physophoridae and Sagitta, speaking highly both of his observations and philosophic power, in the report of the proceedings in Section D.

I have been working in all things with a reference to wide views of zoological philosophy, and the report upon the Echinoderms is intended in common with the mem. on the Salpae to explain my views of Individuality among the lower animals—views which I mean to illustrate still further and enunciate still more clearly in my book that is to be.* They have met with approval from Carpenter, as you will see by the last edition of his *Principles of Physiology*, and I think that Forbes and some others will be very likely eventually to come round to them, but everything that relates to abstract thought is at a low ebb among the mass of naturalists in this country.

In the paper upon “Thalassicolla,” and in that which I read before the British Association, as also in one upon the organisation of the Rotifera, which I am going to have published in the Microscopical Society’s *Transactions*, I have been driving in a series of wedges into Cuvier’s Radiata, and showing how *selon moi* they ought to be distributed.

I am every day becoming more and more certain that you were on the right track thirty years ago in your views of the order and symmetry to be traced in the true natural system.

During the next session I mean to send in a paper to the R.S. upon the “Homologies of the Mollusca,” which shall astonish them. I want to get done for the Mollusca what Savigny did for the Articulata, viz. to show how they all—Cephalopoda, Gasteropoda, Pteropoda, Heteropoda, etc.—are organised on one type, and how the homologous organs are modified in each. What with this and the book, I shall have enough to do for the next six months.

You will doubtless ask what is the practical outlook of all this? whether it leads anywhere in the direction of bread and cheese? To this also I can give a tolerably satisfactory answer.

As you *won’t* have a Professor of Natural History at Sydney—to my great sorrow—I have gone in as a candidate for a Professorial chair at the other end of the world, Toronto in Canada. In England there is nothing to be done—it is the most hopeless prospect I know of; of course the Service offers nothing for me except irretrievable waste of time, and the scientific appointments are so few and so poor that they are not tempting. . . .

Had the Sydney University been carried out as originally proposed, I should certainly have become a candidate for the

* He lectured on this subject at the Royal Institution in 1852.

mind not so great as he thinks himself. He can only work in the concrete from bone to bone, in abstract reasoning he becomes lost—witness “Parthenogenesis” which he told me he considered one of the best things he had done!

He has, however, been very civil to me, and I am as grateful as it is possible to be towards a man with whom I feel it necessary to be always on my guard.

Quite another being is the other leader of Zoological Science in this country—I mean Edward Forbes, Paleontologist to the Geological Survey. More especially a Zoologist and a Geologist than a Comparative Anatomist, he has more claims to the title of a Philosophic Naturalist than any man I know of in England. A man of letters and an artist, he has not merged the *man* in the man of science—he has sympathies for all, and an earnest, truth-seeking, thoroughly genial disposition which win for him your affection as well as your respect. Forbes has more influence by his personal weight and example upon the rising generation of scientific naturalists than Owen will have if he write from now till Doomsday.

Personally I am greatly indebted to him (though the opinion I have just expressed is that of the world in general). During my absence he superintended the publication of my paper, and from the moment of my arrival until now he has given me all the help one man can give another. Why he should have done so I do not know, as when I left England I had only spoken to him once.

The rest of the naturalists stand far below these two in learning, originality, and grasp of mind. Goodsir of Edinburgh should I suppose come next, but he can't write intelligibly. Darwin might be anything if he had good health. Bell is a good man in all the senses of the word, but wants qualities 2 and 3. Newport is a laborious man, but wants 1 and 3. Grant and Rymer Jones—*arcades ambo*—have mistaken their vocation.

My old chief Richardson is a man of men, but troubles himself little with anything but detail zoology. What think you of his getting married for the third time just before his last expedition? I hardly know by which step he approved himself the bolder man.

I think I have now fulfilled my promise of supplying you with a little scientific scandal—and if this long epistle has repaid your trouble in getting through it, I am content.

Believe me, I have not forgotten, nor ever shall forget, your kindness to me at a time when a little appreciation and encour-

wards. This is all as it should be. I can reverence such a man and yet respect myself.

I have been aspiring to great honours since I wrote to you last, to wit the F.R.S., and found no little to my astonishment that I had a chance of it, and so went in. I must tell you that they have made the admission more difficult than it used to be. Candidates are not elected by the Society alone, but fifteen only a year are selected by a committee, and then elected as a matter of course by the Society. This year there were thirty-eight candidates. I did not expect to come in till next year, but I find I am one of the selected. I fancy I shall be the junior Fellow by some years. Singularly enough, among the non-selected candidates were Ward, the man who conducted the Botanical Honours Examination of Apothecaries' Hall nine years ago, and Bryson, the surgeon of the *Fisguard*, i.e. nominally my immediate superior, and who, as he frequently acts as Sir Wm. Burnett's deputy, *will very likely examine me when I pass for Surgeon R.N.!!* That is awkward and must be annoying to him, but it is not my fault. I did not ask for a single name that appeared upon my certificate. Owen's name and Carpenter's, which were to have been appended, were not added. Forbes, my recommender, told me beforehand not to expect to get in this year, and did not use his influence, and so I have no intriguing to reproach myself with or to be reproached with. The only drawback is that it will cost me £14, which is more than I can very well afford.

By the way, I have not told you that after staying for about five months with George, I found that if I meant to work in earnest his home was not the place, so, much to my regret,—for they made me very happy there,—I summoned resolution and *The Boy's Own Book* and took a den of my own, whence I write at present. You had better, however, direct to George, as I am going to move and don't know how long I may remain at my next habitation. At present I am living in the Park Road, but I find it too noisy and am going to St. Anne's Gardens, St. John's Wood, close to my mother's, against whose forays I shall have to fortify myself.

It was a minor addition to his many troubles that after a time Huxley found a grudging and jealous spirit exhibited in some quarters towards his success, and influence used to prevent any further advance that might endanger the existing balance of power in the scientific world. But this could

greediness. And yet withal you will smile at my perversity. I have a certain pleasure in overcoming these obstacles, and fighting these folks with their own weapons. I do so long to be able to trust men implicitly. I have such a horror of all this literary pettifogging. I could be so content myself, if the necessity of making a position would allow it, to work on anonymously, but — I see is determined not to let either me or any one else rise if he can help it. Let him beware. On my own subjects I am his master, and am quite ready to fight half a dozen dragons. And although he has a bitter pen, I flatter myself that on occasions I can match him in that department also.

But I was telling you how busy I am. I am getting a memoir ready for the Zoological Society, and working at my lecture for the Royal Institution, which I want to make striking and original, as it is a good opportunity, besides doing a translation now and then for one of the Journals. Besides this, I am working at the British Museum to make a catalogue of some creatures there. All these things take a world of time and labour, and yield next to no direct profit; but they bring me into contact with all sorts of men, in a very independent position, and I am told, and indeed hope, that something must arise from it. So fair a prospect opens out before me if I can only wait. I am beginning to know what *work* means, and see how much more may be done by steady, unceasing, and well-directed efforts. I thrive upon it too. I am as well as ever I was in my life, and the more I work the better my temper seems to be.

April 30, 1852, 11½ P.M.

I have just returned from giving my lecture * at the Royal Institution, of which I told you in my last letter.

I had got very nervous about it, and my poor mother's death had greatly upset my plans for working it out.

It was the first lecture I had ever given in my life, and to what is considered the best audience in London. As nothing ever works up my energies but a high flight, I had chosen a very difficult abstract point, in my view of which I stand almost alone. When I took a glimpse into the theatre and saw it full of faces, I did feel most amazingly uncomfortable. I can now quite understand what it is to be going to be hanged, and nothing but the necessity of the case prevented me from running away.

* "On Animal Individuality," *Scientific Memoirs*, vol. i. p. 146, cp. p. 88, *supra*.

May 3, 1852.

So much occupation has crowded upon me between the beginning of this letter and the present time that I have been unable to finish it. I had undertaken to give a lecture at the Royal Institution on the 30th April. It was on a difficult subject, requiring a good deal of thought; and as it was my first appearance and before the best audience in London, you may imagine how anxious and nervous I was, and how completely I was obliged to abstract my thoughts from everything else.

However, I am happy to say it is well over. There was a very good audience—Faraday, Prof. Forbes, Dr. Forbes, Wharton Jones, and [a] whole lot of “nobs,” among my auditors. I had made up my mind all day to break down, and then go and hang myself privately. And so you may imagine that I entered the theatre with a very pale face, and a heart beating like a sledge-hammer nineteen to the dozen. For the first five minutes I did not know very clearly what I was about, but by degrees I got possession of myself and of my subject, and did not care for anybody. I have had “golden opinions from all sorts of men” about it, so I suppose I may tell you I have succeeded. I don’t think, however, that I ever felt so thoroughly used up in my life as I did for two days afterwards. There is one comfort, I shall never be nervous again about any audience; but at one’s first attempt, to stand in the place of Faraday and such big-wigs might excuse a little weakness.

The way is clear before me, if my external circumstances will only allow me to persevere; but I fully expect that I shall have to give up my dreams.

Science in England does everything—but *pay*. You may earn praise but not pudding.

I have helping hands held out to me on all sides, but there is nothing to help me to. Last year I became a candidate for a Professorship at Toronto. I took an infinity of trouble over the thing, and got together a mass of testimonials and recommendations, much better than I had any right to expect. From that time to this I have heard nothing of the business—a result for which I care the less, as I believe the chair will be given to a brother of one of the members of the Canadian ministry, who is, I hear, a candidate. Such a qualification as that is, of course, better than all the testimonials in the world.

I think I told you when I last wrote that I was expecting a grant from Government to publish the chief part of my work, done while away. I am expecting it still. I got tired of waiting

As it is I have waited for official confirmation and a convenient season.

And now . . . shall I be very naughty and make a confession? The thing that a fortnight ago (before I got it) I thought so much of, I give you my word I do not care a pin for. I am sick of it and ashamed of having thought so much of it, and the congratulations I get give me a sort of internal sardonic grin. I think this has come about partly because I did not get the official confirmation of what I had heard for some days, and with my habit of facing the ill side of things I came to the conclusion that Weld had made a mistake, and I went in thought through the whole enormous mortification of having to explain to those whom I had mentioned it that it was quite a mistake. I found that all this, when I came to look at it, was by no means so dreadful as it seemed—quite bearable in short—and then I laughed at myself and have cared nothing about the whole concern ever since. In truth . . . I do not think that I am in the proper sense of the word ambitious. I have an enormous longing after the highest and best in all shapes—a longing which haunts me and is the demon which ever impels me to work, and will let me have no rest unless I am doing his behests. The honours of men I value so far as they are evidences of power, but with the cynical mistrust of their judgment and my own worthiness, which always haunts me, I put very little faith in them. Their praise makes me sneer inwardly. God forgive me if I do them any great wrong.

. . . I feel and know that all the rewards and honours in the world will ever be worthless for me as soon as they are obtained. I know that always, as now, they will make me more sad than joyful. I know that nothing that could be done would give me the pure and heartfelt joy and peace of mind that your love has given me, and, please God, shall give for many a long year to come, and yet my demon says work! work! you shall not even love unless you work.

Not blinded by any vanity, then, I hope . . . but viewing this stroke of fortune as respects its public estimation only, I think I must look upon the award of this medal as the turning-point of my life, as the finger-post teaching me as clearly as anything can what is the true career that lies open before me. For whatever may be my own private estimation of it, there can be no doubt as to the general feeling about this thing, and in case of my candidature for any office it would have the very greatest weight. And as you will have seen by my last letter,

a new spirit of anatomical inquiry, not wholly free from a timorous apprehension as to its complete validity." * For the difference between this and the labours of the greatest English comparative anatomist of the time, whose detailed work was of the highest value, but whose generalisations and speculations, based on the philosophy of Oken, proved barren and fruitless, lay in the fact that Huxley, led to it doubtless by his solitary readings in his Charing Cross days, had taken up the method of von Baer and Johannes Müller, then almost unknown, or at least unused in England—"the method which led the anatomist to face his problems in the spirit in which the physicist faced his."

He had been warned by Forbes not to speak too strongly about the dilatoriness of the Government in the matter of the grant, so he writes: "I will 'roar you like any sucking dove' at the dinner, though I felt tempted otherwise." On December 1 he tells how he carried out this advice.

MY DEAR FORBES—You will, I know, like to learn how I got on yesterday. The President's address to me had been drawn up by Bell. It was, of course, too flattering, but he had taken hold of the right points in my work—at least I thought so.

Bunsen spoke very well for Humboldt.

There was a capital congregation at the dinner—sixty or seventy Fellows there. . . .

When it came to my turn to return thanks, I believe I made a very tolerable speechification, at least everybody says so. Lord Rosse had alluded to "science having to take care of itself in this country," and in winding up I gave them a small screed upon that text. That you may see I kept your caution in mind, I will tell you as nearly as may be what I said. I told them that I could not conceive that anything I had hitherto done merited the honour of that day (I looked so preciously meek over

* "In these papers (on the Medusæ) you have for the first time fully developed their structure, and laid the foundation of a rational theory for their classification." "In your second paper 'On the Anatomy of Salpa and Pyrosoma,' the phenomena, etc., have received the most ingenious and elaborate elucidation, and have given rise to a process of reasoning, the results of which can scarcely yet be anticipated, but must bear in a very important degree upon some of the most abstruse points of what may be called transcendental physiology." See *Royal Society*, Obituary Notices, vol. lix. p. 1.

just about to make another application to the present Government on the subject. While this business has been dragging on of course I have not been idle. I have four memoirs (on various matters in Comparative Anatomy) in the *Philosophical Transactions*, and they have given me their Fellowship and one of the Royal medals. I have written a whole lot of things for the journals—reviews for the *British and Foreign Quarterly Medical*, etc. I am one of the editors of Taylor's *Scientific Memoirs* (German scientific translations). In conjunction with my friend Busk I am translating a great German book on the *Microscopical Anatomy of Man*, and I have engaged to write a long article for Todd's *Cyclopædia*. Besides this, have read two long memoirs at the British Association, and have given two lectures at the Royal Institution—one of them only two days ago, when I was so ill with influenza I could hardly stand or speak.

Furthermore, I have been a candidate for a Professorship of Natural History at Toronto (which is not even yet decided); for one at Aberdeen, which has been given against me; and at present I am a candidate for the Professorship of Physiology at King's College, or, rather, for half of it—Todd having given up, and Bowman, who remains, being willing to take only half, and that he will soon give up. My friend Edward Forbes—a regular brick, who has backed me through thick and thin—is backing me for King's College, where he is one of the Professors. My chance is, I believe, very good, but nothing can be more uncertain than the result of the contest. If they don't take one of their own men I think they will have me. It would suit me very well, and the whole chair is worth £400 a year, and would enable me to live.

Something I must make up my mind to do, and that speedily. I can get honour in Science, but it doesn't pay, and "honour heals no wounds." In truth I am often very weary. The longer one lives the more the ideal and the purpose vanishes out of one's life, and I begin to doubt whether I have done wisely in giving vent to the cherished tendency towards Science which has haunted me ever since my childhood. Had I given myself to Mammon I might have been a respectable member of society with large watch-seals by this time. I think it is very likely that if this King's College business goes against me, I may give up the farce altogether—burn my books, burn my rod, and take to practice in Australia. It is no use to go on kicking against the pricks. . . .

CHAPTER VIII

1854

THE year 1854 marks the turning-point in Huxley's career. The desperate time of waiting came to an end. By the help of his lectures and his pen, he could at all events stand and wait independently of the Navy. He could not, of course, think of immediate marriage, nor of asking Miss Heathorn to join him in England; but it so happened that her father was already thinking of returning home, and finally this was determined upon just before Professor Forbes' translation to a chair at Edinburgh gave Huxley what turned out to be the long-hoped-for permanency in London.

June 3, 1854.

I have often spoken to you of my friend Edward Forbes. He has quite recently been suddenly appointed to a Professorial Chair in Edinburgh, vacated by the death of old Jamieson. He was obliged to go down there at once and lecture, and as he had just commenced his course at the Government School of Mines in Jermyn Street, it was necessary to obtain a substitute. He had spoken to me of the possibility of his being called away long ago, and had asked if I would take his place, to which, of course, I assented, but the whole affair was so uncertain that I never in any way reckoned upon it. Even at last I did not know on the Monday whether I was to go on for him on the Friday or not. However, he did go after giving two lectures, and on Friday the 25th May I took his lecture, and I have been going on ever since, twice a week on Mondays and Fridays. Called upon so very suddenly to give a course of some six and twenty lectures, I find it very hard work, but I like it and I never was in better health.

On July 20, this temporary work, which he had undertaken as the friend of Forbes, was exchanged for one of the permanent lectureships formerly held by the latter. A hundred a year for twenty-six lectures was not affluence; it would have suited him better to have had twice the work and twice the pay. But it was his crossing of the Rubicon, and, strangely enough, no sooner had he gained this success than it was doubled.

July 30, 1854.

I was appointed yesterday to a post of £200 a year. It has all come about in the strangest way. I told you how my friend Forbes had been suddenly called away to Edinburgh, and that I had suddenly taken his duties—sharp work it has been I can tell you these summer months, but it is over and done satisfactorily. Forbes got £500 a year, £200 for a double lectureship, £300 for another office. I took one of the lectureships, which would have given me £100 a year only, and another man was to have the second lectureship and the other office in question. It was so completely settled a week ago that I had written to the President of the Board of Trade who makes the appointment, accepting mine, and the other man had done the same. Happily for me, however, my new colleague was suddenly afflicted with a sort of moral colic, an absurd idea that he could not perform the duties of his office, and resigned it. The result is that a new man has been appointed to the office he left vacant, while the lectureship was offered to me. Of course I took it, and so in the course of the week I have seen my paid income doubled. . . . So after a short interval I have become a Government officer again, but in rather a different position I flatter myself. I am chief of my own department, and my position is considered a very good one—as good as anything of its kind in London.

Furthermore, on August 11 he was “entrusted with the Coast Survey investigations under the Geological Survey, and remunerated by fee until March 31, 1855, when he was ranked as Naturalist on the Survey with an additional salary of £200, afterwards increased to £400, rising to £600 per annum,” as the official statement has it.

Then in quick succession he was offered in August a lectureship on Comparative Anatomy at St. Thomas’ Hospital for the following May and June, and in September he

injudicious to bring you forward, and that, as you were named, I for my own part should not have brought forward Forbes as a candidate; that therefore while willing to speak up to any extent for Forbes' *positive* merits and deserts, I would carefully be understood to give no opinion as to your and his *relative* standing.

They did not take much by my speech therefore either way, more especially as I voted for *both* of you.

I hate doing anything of the kind "unbeknownst" to people, so there is the exact history of my proceedings. If I had been able to come to the clear conclusion that the claims of either of you were strongly superior to those of the other, I think I should have had the honesty and moral courage to "act accordin'," but I really had not, and so there was no part to play but that of a sort of Vicar of Bray.—Ever yours faithfully,

T. H. HUXLEY.

Forbes' reply was a letter which Huxley, after his friend's death, held "among his most precious possessions." It appeared without names in the obituary notice of Forbes in the *Literary Gazette* for November 25, 1854, as an example of his unselfish generosity:—

I heartily concur in the course you have taken, and had I been placed as you have been, would have done exactly the same. . . . Your way of proceeding was as true an act of friendship as any that could be performed. As to myself, I dream so little about medals, that the notion of being on the list never entered my brain, even when asleep. If it ever comes I shall be pleased and thankful; if it does not, it is not the sort of thing to break my equanimity. Indeed, I would always like to see it given not as a mere honour, but as a help to a good man, and this it is assuredly in Hooker's case. Government people are so ignorant that they require to have merits drummed into their heads by all possible means, and Hooker's getting the medal may be of real service to him before long. I am in a snug, though not an idle, nest,—he has not got his resting-place yet. And so, my dear Huxley, I trust that you know me too well to think that I am either grieved or envious, and you, Hooker, and I are much of the same way of thinking.

It is interesting to record the same scrupulosity over the election to the Registrarship of the University of London in 1856, when, having begun to canvass for Dr. Latham

Natural History Sciences.* This, when it came out later as a pamphlet, he sent to his Tenby friend Dr. Dyster (of whom hereafter), to whose criticism on one passage he replied on October 10:—

. . . —I am rejoiced you liked my speechment. It was written hastily and is, like its speaker, I fear, more forcible than eloquent, but it can lay claim to the merit of being sincere.

My intention on p. 28 was by no means to express any satisfaction at the worms being as badly off as ourselves, but to show that pain being everywhere is inevitable, and therefore like all other inevitable things to be borne. The rest of it is the product of my scientific Calvinism, which fell like a shell at your feet when we were talking over the fire.

I doubt, or at least I have no confidence in, the doctrine of ultimate happiness, and I am more inclined to look the opposite possibility fully in the face, and if that also be inevitable, make up my mind to bear it also.

You will tell me there are better consolations than Stoicism; that may be, but I do not possess them, and I have found my "grin and bear it" philosophy stand me in such good stead in my course through oceans of disgust and chagrin, that I should be loth to give it up.

The summer of 1854 was spent in company with the Busks at Tenby, amid plenty of open-air work and in great peace of mind, varied with a short visit to Liverpool in order to talk business with his friend Forbes, who was eager that Huxley should join him in Edinburgh.

* The subsequent reference is to the words, "I cannot but think that he who finds a certain proportion of pain and evil inseparably woven up in the life of the very worms will bear his own share with more courage and submission; and will, at any rate, view with suspicion those weakly amiable theories of the divine government, which would have us believe pain to be an oversight and a mistake, to be corrected by and by." (*Collected Essays*, iii. p. 62.) This essay contains the definition of science as "trained and organised common sense," and the reference to a new "Peter Bell" which suggested Miss May Kendall's spirited parody of Wordsworth:—

Primroses by the river's brim
Dicotyledons were to him,
And they were nothing more.

what advice to give you. When I wrote I hardly knew what kind of work you had in your present office, but Francis has since enlightened me. I thought you had more leisure. One thing is very clear—you must come out of that. Your Pegasus is quite out of place ploughing. You are using yourself up in work that comes to nothing, and so far as I can see cannot be worse off.

Now what are your prospects? Why, as I told you before, you have made a *succès* here and must profit by it. The other night your name was mentioned at the Philosophical Club (the most influential scientific body in London) with great praise. Gassiot, who has great influence, said in so many words, “you had made your fortune,” and I frankly tell you I believe so too, if you can only get over the next three years. So you see that *quoad* position, like Quintus Curtius, there is a “fine opening” ready for you, only mind you don’t spoil it by any of your horrid modesty.

So much for glory—now for economics. I have been trying to ferret out more nearly your chances of a post, and here are my results (which, I need not tell you, must be kept to yourself).

At the Museum in Jermyn Street, Playfair, Forbes, Percy and I think Sir Henry would do anything to get you, and eliminate —; but, so far as I can judge, the probability of his going is so small that it is not worth your while to reckon upon it. Nevertheless it may be comforting to you to know that in case of anything happening these men will help you tooth and nail. Cultivate Playfair when you have a chance—he is a good fellow, wishes you well, has great influence, and will have more. *Entre nous*, he has just got a new and important post under Government.

Next, the Royal Institution. This is where, as I told you, you ought to be—looking to Faraday’s place. Have no scruple about your chemical knowledge; you won’t be required to train a college of students in abstruse analyses; and if you were, a year’s work would be quite enough to put you at ease. What they want, and what you have, are *clear powers of exposition*—so clear that people may think they understand even if they don’t. That is the secret of Faraday’s success, for not a tithe of the people who go to hear him really understand him.

However, I am afraid that a delay must occur before you can get placed at the Royal Institution, as you cannot hold the Professorship until you have given a course of lectures there,

to whom I can feel obliged, without losing a particle of independence or self-respect.

The following from a letter to Hooker, announcing Forbes' death, is a striking testimony to his worth:—

I think I have never felt so crushed by anything before. It is one of those losses which cannot be replaced either to the private friend or to science. To me especially it is a bitter loss. Without the aid and sympathy he has always given me from first to last, I should never have had the courage to persevere in the course I have followed. And it was one of my greatest hopes that we should work in harmony for long years at the aims so dear to us both.

But it is otherwise, and we who remain have nothing left but to bear the inevitable as we best may.

And again a few days later:—

I have had no time to write to you again till now, but I write to say how perfectly you express my own feeling about our poor friend. One of the first things I thought of was that medal business,* and I never rejoiced in anything more than that I had not been deterred by any moral cowardice from acting as I did.

As it is I reckon that letter (which I will show you some day) among my most precious possessions.

Huxley's last tribute to his dead friend was the organising a memorial fund, part of which went to getting a bust of him made, part to establishing an Edward Forbes medal, to be competed for by the students of his old school in Jermyn Street.

As Huxley had been Forbes' successor at Jermyn Street, so now he seemed to many marked out to succeed him at Edinburgh. In November he writes to Hooker:—

People have been at me about the Edinburgh chair. If I could contrive to stop here, between you and I, I would prefer it to half a dozen Edinburgh chairs, but there is a mortal difference between £200 and £1000 a year. I have written to say that if the Professors can make up their minds they wish me to stand, I will—if not, I will not. For my own part, I believe my chances

* P. 119.

to the labour of translation—and I could get any amount of scientific work I wanted—so there was a living, though a scanty one, and amazingly hard work for it. My pen is not a very facile one, and what I write costs me a good deal of trouble.

In the spring of this year, however, a door opened. My poor lost friend Professor Forbes—whose steady attachment and aid had always been of the utmost service to me—was called to fill the chair of Natural History in Edinburgh at a moment's notice. It is a very valuable appointment, and he was obliged to fill it at once. Of course he left a number of vacancies behind, among them one at the Government School of Mines in Jermyn Street, where he lectured on Natural History. I was called upon to take up his lectures where he left off, in the same sudden way, and the upshot of it all was that I became permanently attached—with £200 a year pay. In other ways I can make a couple of hundred a year more even now, and I hope by-and-by to do better. In fact, a married man, as I hope soon to be, cannot live at all in the position which I ought to occupy under less than six hundred a year. If I keep my health, however, I have every hope of being able to do this—but, as the jockeys say, the pace is severe. Nettie is coming over in the spring, and if I have any luck at all, I mean to have paid off my debts and to be married by this time next year.*

In the meanwhile, strangely enough—and very painfully for me—new possibilities have sprung up. My poor friend Forbes died only a week ago, just as he was beginning his course and entering upon as brilliant a career as ever was opened to any scientific man in this country.

I cannot tell you how deeply this has shocked me. I owe him so much, I loved him so well, and I have so very very few friends in the true sense of the word, that it has been perhaps

* He writes on July 21, 1851:—"I commenced life upon nothing at all, and I had to borrow in the ordinary way from an agent for the necessary expenses of my outfit. I sent home a great deal of money, but notwithstanding, from the beautiful way they have of accumulating interest and charges of one description and another, I found myself £100 in debt when I returned—besides something to my brother, about which, however, I do not suppose I need trouble myself just at present. As you may imagine, living in London, my pay now hardly keeps me, to say nothing of paying off my old scores. I could get no account of how things were going on with my agent while I was away, and therefore I never could tell exactly how I stood."

ends in the book line, among other things a Shakespeare for yourself, dear Liz.—Believe me, ever your affec. brother,
T. H. HUXLEY.

In December the Edinburgh chair was practically offered to him undivided; but by that time the London authorities thought they had better make it worth his while to stay at Jermyn Street, and with negotiations begun for this end he refused to stand for Edinburgh. In the following spring, however, he was again approached from Edinburgh—not so much to withdraw his refusal and again become a candidate, as to let it be made known that he would accept the chair if it were offered him. But his position in London was now established; and he preferred to live in London on a bare sufficiency rather than to enjoy a larger income away from the centre of things.

Two letters to Tyndall, which refer to the division of labour in the science reviews for the *Westminster* (see p. 92), indicate very clearly the high pressure at which Huxley had already begun to work:—

TENBY, SOUTH WALES, *Oct.* 22, 1854.

MY DEAR TYNDALL—I was rejoiced to find you entertaining my proposition at all. No one believes how hard you work more than I, but I was not going to be such a bad diplomatist as to put that at the head of my letter, and if I had thought that what I want you to do involved any great accession thereto, I think I could not have mustered up the face to ask you. But really and truly, so long as it is confined to our own department it is no great affair. You make me laugh at the long face you pull about the duties, based on my phrase. The fact is, you notice what you like, and what you do not you leave undone, unless you get an editorial request to say something about a particular book. The whole affair is entirely in your own hands—at least it is in mine—as I went upon my principle of having a row at starting. . . .

Now here is an equitable proposition. Look at my work. I have a couple of monographs, odds and ends of papers for journals, a manual and some three courses of lectures to provide for this winter. “My necessities are as great as thine,” as Sir Philip Sidney didn’t say, so be a brick, split the difference, and

interest in marine zoology was the starting-point of a warm friendship with the rising naturalist, some fifteen years his junior. He was strongly urged by the younger man to complete and systematise his observations by taking in turn all the species of each genus of annelids found at Tenby, and working them up into a series of little monographs "which would be the best of all possible foundations for a History of the British Annelidæ":—

TO DR. DYSTER

Jan. 5, 1855.

[He begins by confessing "a considerable liberty" he had been taking with Dyster's name, in calling a joint discovery of theirs, which he described in the *Edinburgh New Philosophical Journal*, *Protula Dysteri*.]

Are you very savage? If so, you must go and take a walk along the sands and see the slant rays of the sunset tipping the rollers as they break on the beach; that always made even *me* at peace with all the world, and *a fortiori* it will you.

Truly, I wish I had any such source of consolation. Chimney pots are highly injurious to my morals, and my temper is usually in proportion to the extent of my horizon.

I have been swallowing oceans of disgust lately. All sorts of squabbles, some made by my own folly and others by the malice of other people, and no great sea and sky to go out under, and be alone and forget it all.

You may have seen my name advertised by Reeve as about to write a memoir of poor Forbes, to be prefixed to a collection of his essays. I found that to be a mere bookseller's dodge on Reeve's part, and when I made the discovery, of course we had a battle-royal, and I have now wholly withdrawn from it.

I find, however, that one's kind and generous friends imagine it was an electioneering manœuvre on my part for Edinburgh. Imagine how satisfactory. I forget whether I told you that I had been asked to stand for Edinburgh and have done so. Whether I shall be appointed or not I do not know. So far as

help), and later to Charles Kingsley, whom he first met at the end of June 1855. "What Kingsley do you refer to?" he writes on May 6, "*Alton Locke* Kingsley or Photographic Kingsley? I shall be right glad to find good men and true anywhere, and I will take your bail for any man. But the work must be critically done."

and if I had obtained the Edinburgh chair, I should have been in reality a mere pedagogue and a man of science only in name. Such were my notions, and if I hesitated at all and allowed myself to become a candidate, it was only because I have other interests to consult than my own. Intending to "range myself" one of these days and become a respectable member of society, I was bound to consider my material interests. And so I should have been still a candidate for Edinburgh had not the Government here professed themselves unwilling to lose my services, adding the "material guarantee" of an addition to my income, which, though by no means bringing it up to the point of Edinburgh, will still enable me (*das heisst* "us") to live comfortably here.

I must renounce the "poms and vanities," but all those other "lusts of the flesh" which may beseem a gentleman may be reasonably gratified.

Don't you think I have been wise in my Hercules choice? After all I don't lay claim to any great merit, seeing it was anything but certain I should get Edinburgh.

The best of all is that I have every reason to believe that Government will carry out my scheme for a coast survey, so happily and pleasantly begun at Tenby last year.

The final arrangements are almost complete, and I believe you may make up your mind to have four months of me next year. Tenby shall be immortalised and Jenkyn* converted into a philosopher. By the way, I think the best way would be to retain the shells till I come. My main purpose is to have in them a catalogue of what Tenby affords.

Pray give my kind remembrances to Mrs. Dyster, and believe me, ever yours,

T. H. HUXLEY.

April 1, 1855.

MY DEAR DYSTER—By all that's good, your last note, which lies before me, has date a month ago. I looked at it just now, and became an April fool on the instant.

All the winds of March, however, took their course through my thorax and eventuated in lectures. At least that is all the account I can give to myself of the time, and an unprofitable account it is, for everything but one's exchequer.

So far as knowledge goes it is mere prodigality spending

* Jenkyn was employed to collect shells, etc., at Tenby. He is often alluded to as "the Professor."

April 9, 1855.

MY DEAR DYSTER — I didn't by any means mean to be so sphinx-like in my letter, though you have turned out an Œdipus of the first water. True it is that I mean to "range myself," "live cleanly and leave off sack," within the next few months—that is to say, if nothing happen to the good ship which is at present bearing my fiancée homewards.

So far as a restless mortal—more or less weary of most things—like myself can be made happy by any other human being, I believe your good wishes are safe of realisation; at any rate, it will be my fault if they are not, and I beg you never to imagine that I could confound the piety of friendship with the "efflorescent" variety.

I hope to marry in July, and make my way down to Tenby shortly afterwards, and I am ready to lay you a wager that your vaticinations touching the amount of work that *won't* be done don't come true.

So much for wives—now for *worms*—(I could not for the life of me help the alliteration). I, as right reverend father in worms and Bishop of Annelidæ, do not think I ought to interfere with my most promising son, when a channel opens itself for the publication of his labours. So do what you will *apropos* of J——. If he does not do the worms any better than he did the zoophytes, he won't interfere with my plans.

I shall be glad to see Mrs. Buckland's Echinoderm. I think it must be a novelty by what you say. She is a very jolly person, but I have an unutterable fear of scientific women.—
Ever yours, T. H. HUXLEY.

May 6, 1855.

My ship is not come home but is coming, and I have been in a state of desperation at the continuous east winds. However, to-day there is a westerly gale, and if it lasts I shall have news soon. You may imagine that I am in an unsatisfactory state of mind between this and lecturing five times a week.

I beg to say that the "goods" I expect are home produce transplanted (or sent a voyage as you do Madeira), and not foreign growth by any means. But it is five years since we met, I am another man altogether, and if my wife be as much altered, we shall need a new introduction. Correspondence, however active, is a poor substitute for personal communication and tells one but little of the inner life.

Finally, on the eve of his marriage in July, Tyndall congratulates him on being appointed to deliver the next course of Fullerian Lectures at the Royal Institution:—

The fates once seemed to point to our connection in a distant land: we are now colleagues at home, and I can claim you as my scientific brother. May the gods continue to drop fatness upon you, and may your next great step be productive of all the felicity which your warmest friends or your own rebellious heart can desire.

CHAPTER IX

1855

MISS HEATHORN and her parents reached England at the beginning of May 1855, and took up their abode at 8 Titchfield Terrace, not far from Huxley's own lodgings and his brother's house. One thing, however, filled Huxley with dismay. Miss Heathorn's health had broken down utterly, and she looked at death's door. All through the preceding year she had been very ill; she had gone with friends, Mr. and Mrs. Wise, to the newly opened mining-camp at Bathurst, and she and Mrs. Wise were indeed the first women to visit it; returning to Sydney after rather a rough time, she caught a chill, and being wrongly treated by a doctor of the blood-letting, calomel-dosing school, she was reduced to a shadow, and only saved by another practitioner, who reversed the treatment just in time.

In his letters to her, Huxley had not at first realised the danger she had been in; and afterwards tried to keep her spirits up by a cheerful optimism that would only look forward to their joyful union and many years of unbroken happiness to atone for their long parting.

But the reality alarmed him. He took her to one of the most famous doctors of the day, as if merely a patient he was interested in. Then as one member of the profession to another, he asked him privately his opinion of the case. "I give her six months of life," said Æsculapius. "Well, six months or not," replied Huxley, "she is going to be my wife." The doctor was mightily put out. "You ought to have told me that before." Of course, the evasive answer in such a contingency was precisely what Huxley wished to

It was not a large party that assembled at the George Huxleys for the wedding, but all were life-long friends, including, besides the Fanning clan and Mrs. Griffiths, an old Australian ally, Hooker, Tyndall, and Dr. and Mrs. Carpenter. There was none present but felt that abundant happiness was at least well earned after eight years of trial, and still more that its best guarantee was the firm loyalty and devotion that had passed through so many dangers of absence and isolation, so many temptations to renounce the ideal course under stress of circumstance, only to emerge strengthened and ennobled by the stern discipline of much sacrifice.

Great as was his new happiness, he hardly stood in need of Darwin's word of warning: "I hope your marriage will not make you idle; happiness, I fear, is not good for work." Huxley could not sit idle for long. If he had no occupation on hand, something worth investigation—and thorough investigation—was sure to catch his eye. So he writes to Hooker from Tenby:—

15 ST. JULIAN'S TERRACE, TENBY,
Aug. 16, 1855.

MY DEAR HOOKER—I am so near the end of the honeymoon that I think it can hardly be immodest if I emerge from private life and write you a letter, more particularly as I want to know something. I went yesterday on an expedition to see the remains of a forest which exists between tidemarks at a place called Amroth, near here.

So far as I can judge there can be no doubt that this really is a case of downward movement. The stools of the trees are in their normal position, and their roots are embedded and interwoven in a layer of stiff blue clay, which lies immediately beneath the superficial mud of the shore. Layers of leaves, too, are mixed up with the clay in other parts, and the bark of some of the trees is in perfect preservation. The condition of the wood is very curious. It is like very hard cheese, so that you can readily cut slices with a spade, and yet where more of the trunk has been preserved some parts are very hard. The trees are, I fancy, Beech and Oak. Could you identify slices if I were to send you some?

Now it seems to me that here is an opportunity one does not often have of getting some information about the action of

sea water on wood, and on the mode in which these vegetable remains may become embedded, etc. etc., and I want to get you to tell me where I can find information on submerged forests in general, so as to see to what points one can best direct one's attention, and to suggest any inquiries that may strike yourself.

I do not see how the stumps can occur in this position without direct sinking of the land, and that such a sinking should have occurred tallies very well with some other facts which I have observed as to the nature of the bottom at considerable depths here.

We had the jolliest cruise in the world by Oxford, Warwick, Kenilworth, Stratford, Malvern, Ross, and the Wye, though it *was* a little rainy, and though my wife's strength sadly failed at times.

Still she was on the whole much better and stronger than I had any right to expect, and although I get frightened every now and then, yet there can be no doubt that she is steadily though slowly improving. I have no fears for the ultimate result, but her amendment will be a work of time. We have really quite settled down into Darby and Joan, and I begin to regard matrimony as the normal state of man. It's wonderful how light the house looks when I come back weary with a day's boating to what it used to do.

I hope Mrs. Hooker is well and about again. Pray give her our very kind regards, and believe me, my dear Hooker, ever yours,

T. H. HUXLEY.

At Tenby he stayed on through August and September, continuing his occupations of the previous summer, dredging up specimens for his microscope, and working partly for his own investigations, partly for the Geological Survey.

CHAPTER X

1855-1858

UP to his appointment at the School of Mines, Huxley's work had been almost entirely morphological, dealing with the Invertebrates. His first investigations, moreover, had been directed not to species-hunting, but to working out the real affinities of little known orders, and thereby evolving a philosophical classification from the limbo of "Vermes" and "Radiata."

He had continued the same work by tracing homologies of development in other classes of animals, such as the Cephalous Mollusca, the Articulata, and the Brachiopods. On these subjects, also, he had a good deal of correspondence with other investigators of the same cast of mind, and even when he did not carry conviction, the impression made by his arguments may be judged from the words of Dr. Allman, no mean authority, in a letter of May 2, 1852:—

I have thought over your arguments again and again, and while I am the more convinced of their ingenuity, originality, and strength, I yet feel ashamed to confess that I too must exclaim "*tenax propositi*." When was it otherwise in controversy?

Other speculations arising out of these researches had been given to the public in the form of lectures, notably that on Animal Individuality at the Royal Institution in 1852.

But after 1854, Paleontology and administrative work began to claim much of the time he would willingly have bestowed upon distinctly zoological research. His lectures

writing of "introductions" to each section of the catalogue, which should be a guide to the students. The "Method of Paleontology" mentioned above served as the prefatory essay to the whole catalogue, and was reprinted in 1869 by the Smithsonian Institute of Washington under the title of *Principles and Methods of Paleontology*.

This work led to his taking a lively interest in the organisation of museums in general, whether private, such as Sir Philip Egerton's, which he visited in 1856; local, such as Warwick or Chester; or central, such as the British Museum or that at Manchester.

With regard to the British Museum, the question had arisen of removing the Natural History collections from the confined space and dusty surroundings of Great Russell Street. A first memorial on the subject had been signed, not only by many non-scientific persons, but also by a number of botanists, who wished to see the British Museum Herbarium, etc., combined with the more accessible and more complete collections at Kew. Owing apparently to official opposition, the Natural History sub-committee of the British Museum Trustees advised a treatment of the Botanical Department which commended itself to none of the leading botanists. Consequently a number of botanists and zoologists took counsel together and drew up a fresh memorial from the strictly scientific point of view. Huxley and Hooker took an active part in the agitation. "It is no use," writes the former to his friend, "putting any faith in the old buffers, hardened as they are in trespasses and sin." And again:—

I see nothing for it but for you and I to constitute ourselves into a permanent "Committee of Public Safety," to watch over what is being done and take measures with the advice of others when necessary. . . . As for — and *id genus omne*, I have never expected anything but opposition from them. But I don't think it is necessary to trouble one's head about such opposition. It may be annoying and troublesome, but if we are beaten by it we deserve to be. We shall have to wade through oceans of trouble and abuse, but so long as we gain our end, I care not a whistle whether the sweet voices of the scientific mob are with me or against me.

within twenty years the ideal here sketched was to a great extent realised, as any visitor to the Natural History Museum at South Kensington can see for himself.

The same principles are reiterated in his letter of January 25, 1868, to the Commissioners of the Manchester Natural History Society, who had asked his advice as to the erection of a museum. But to the principles he adds a number of most practical suggestions as to the actual structure of the building, which are briefly appended in abstract. The complement to this is a letter of 1872, giving advice as to a local museum at Chester, and one of 1859 describing the ideal catalogue for a geological museum.

Jan. 25, 1868.

The Commissioners of the Manchester
Natural History Society.

Scheme for a Museum.

Objects.—1. The public exhibition of a collection of specimens large enough to illustrate all the most important truths of Natural History, but not so extensive as to weary and confuse ordinary visitors.

2. The accessibility of this collection to the public.

3. The conservation of all specimens not necessary for the purpose defined in (1) in a place apart.

4. The accessibility of all objects contained in the museum to the curator and to scientific students, without interference with the public or by the public.

5. Thorough exclusion of dust and dirt from the specimens.

6. A provision of space for workrooms, and, if need be, lecture-rooms.

Principle.—A big hall ($350 \times 40 \times 30$) with narrower halls on either side, lighted from the top. The central hall for the public, the others for the curators, etc. The walls, of arches upon piers about 15 ft. high, bearing on girders a gallery 5 ft. wide in the public room, and 3 ft. 6 in. in the curators'.

The cases should be larger below, 5 ft. deep, and smaller above, 2 ft. deep, with glass fronts to the public, and doors on the curators' side.

For very large specimens—*e.g.* a whale—the case could expand into the curators' part without encroaching on the public part, so as to keep the line of windows regular.

If I may make a suggestion I should say that a catalogue of your museum for popular use should commence with a sketch of the topography and stratigraphy of the county, put into the most intelligible language, and illustrated by reference to mineral specimens in the cases, and to the localities where sections showing the superposition of such and such beds is to be seen. After that I think should come a list of the most remarkable and interesting fossils, with reference to the cases where they are to be seen; and under the head of each a brief popular account of the kind of animal or plant which the thing was when alive, its probable habits, and its meaning and importance as a member of the great series of successive forms of life.—Yours very faithfully,

T. H. HUXLEY.

The reorganisation of the course of studies at Jermyn Street, fully sketched out in the 1857 notebook, involved two very serious additions to his work over and above what was required of him by his appointment as Professor. He found his students to a great extent lacking in the knowledge of general principles necessary to the comprehension of the special work before them. To enable them to make the best use of his regular lectures, he offered them in addition a preliminary evening course of nine lectures each January, which he entitled "An Introduction to the Study of the Collection of Fossils in the Museum of Practical Geology." These lectures summed up what he afterwards named Physiography, together with a general sketch of fossils and their nature, the classification of animals and plants, their distribution at various epochs, and the principles on which they are constructed, illustrated by the examination of some animal, such as a lobster.

The regular lectures, fifty-seven in number, ran from February to April and from April to June, with fortnightly examinations during the latter period, six in number. I take the scheme from his notebook:—"After prolegomena, the physiology and morphology of lobster and dove; then through Invertebrates, Anodon, Actinia, and Vorticella Protozoa, to Molluscan types. Insects, then Vertebrates. Supplemented Paleontologically by the demonstrations of the selected types in the cases; twelve Paleozoic, twelve

first address at the Working Men's College to an audience, as he notes, of some fifty persons, including Maurice himself.

Other work of importance was connected with the Royal Institution. He had been elected to deliver the triennial course as Fullerian Professor, and for his subject in 1856-57 chose Physiology and Comparative Anatomy; in 1858, the Principles of Biology.

He was extremely glad of the additional "grist to the mill" brought in by these lectures. As he wrote in 1890:—

I have good reason to know what difference a hundred a year makes when your income is not more than four or five times that. I remember when I was candidate for the Fullerian professorship some twenty-three years ago, a friend of mine asked a wealthy manager to support me. He promised, but asked the value of the appointment, and when told, said, "Well, but what's the use of a hundred a year to him?" I suppose he paid his butler that.

A further attempt to organise scientific work throughout the country and make its results generally known, dates from this time. Huxley, Hooker, and Tyndall had discussed, early in 1858, the possibility of starting a *Scientific Review*, which should do for science what the *Quarterly* or the *Westminster* did for literature. The scheme was found not to be feasible at the time, though it was revived in another form in 1860; so in the meanwhile it was arranged that science should be laid before the public every fortnight, through the medium of a scientific column in the *Saturday Review*. The following letter bears on this proposal:—

April 20, 1858.

MY DEAR HOOKER—Before the dawn of the proposal for the ever-memorable though not-to-be *Scientific Review*, there had been some talk of one or two of us working the public up for science through the *Saturday Review*. Maskelyne (you know him, I suppose) was the suggester of the scheme, and undertook to talk to the *Saturday* people about it.

I thought the whole affair had dropped through, but yesterday Maskelyne came to me and to Ramsay with definite propositions from the *Saturday* editor.

two contributions in 1858—one on the general subject of the cell theory, the other on the particular question of the development of the skull. “In a striking ‘Review of the Cell Theory,’” says Sir M. Foster, “which appeared in the *British and Foreign Medical Review* in 1858, a paper which more than one young physiologist at the time read with delight, and which even to-day may be studied with no little profit, he, in this subject as in others, drove the sword of rational inquiry through the heart of conceptions, metaphysical and transcendental, but ‘dominant.’”

Of this article Professor E. Ray Lankester also writes:—

. . . Indeed it is a fundamental study in morphology. The extreme interest and importance of the views put forward in that article may be judged of by the fact that although it is forty years since it was published, and although our knowledge of cell structure has made immense progress during those forty years, yet the main contention of that article, viz. that cells are not the cause but the result of organisation—in fact, are, as he says, to the tide of life what the line of shells and weeds on the sea-shore is to the tide of the living sea—is even now being re-asserted, and in a slightly modified form is by very many cytologists admitted as having more truth in it than the opposed view and its later outcomes, to the effect that the cell is the unit of life in which and through which alone living matter manifests its activities.

The second was his Croonian Lecture of 1858, “On the Theory of the Vertebrate Skull,” in which he demonstrated from the embryological researches of Rathke and others, that after the first step the whole course of development in the segments of the skull proceeded on different lines from that of the vertebral column; and that Oken’s imaginative theory of the skull as modified vertebrae, logically complete down to a strict parallel between the subsidiary head-bones and the limbs attached to the spine, outran the facts of a definite structure common to all vertebrates which he had observed.*

* “Following up Rathke, he strove to substitute for the then dominant fantastic doctrines of the homologies of the cranial elements advocated by Owen, sounder views based on embryological evidence.

CHAPTER XI

1857-1858

THROUGHOUT this period his health was greatly tried by the strain of his work and life in town. Headache! headache! is his repeated note in the early part of 1857, and in 1858 we find such entries as:—"Feb. 11.—Used up. Hypochondriacal and bedevilled." "Ditto 12." "13.—Not good for much." "21.—Toothache, incapable all day." And again:—"March 30.—Voiceless." "31.—Missed lecture." And, "April 1.—Unable to go out." He would come in thoroughly used up after lecturing twice on the same day, as frequently happened, and lie wearily on one sofa; while his wife, whose health was wretched, matched him on the other. Yet he would go down to a lecture feeling utterly unable to deliver it, and, once started, would carry it through successfully—at what cost of nervous energy was known only to those two at home.

But there was another branch of work, that for the Geological Survey, which occasionally took him out of London, and the open-air occupation and tramping from place to place did him no little good. Thus, through the greater part of September and October 1856 he ranged the coasts of the Bristol Channel from Weston to Clovelly, and from Tenby to Swansea, preparing a "Report on the Recent Changes of Level in the Bristol Channel." "You can't think," he writes from Braunton on October 3, "how well I am, so long as I walk eight or ten miles a day and don't work too much, but I find fifteen or sixteen miles my limit for comfort."

For many years after this his favourite mode of recruit-

BERNE, *Sept.* 3, 1856.

I send you a line hence, having forgotten to write from Interlaken, whence we departed this morning.

The Weissthör expedition was the most successful thing you can imagine. We reached the Riffelberg in 11½ hours, the first six being the hardest work I ever had in my life in the climbing way, and the last five carrying us through the most glorious sight I ever witnessed. During the latter part of the day there was not a cloud on the whole Monte Rosa range, so you may imagine what the Matterhorn and the rest of them looked like from the wide plain of *névé* just below the Weissthör. It was quite a new sensation, and I would not have missed it for any amount; and besides this I had an opportunity of examining the *névé* at a very great height. A regularly stratified section, several hundred feet high, was exposed on the Cima di Jazi, and I was convinced that the Weissthör would be a capital spot for making observations on the *névé* and on other correlative matters. There are no difficulties in the way of getting up to it from the Zermatt side, tough job as it is from Macugnaga, and we might readily rig a tent under shelter of the ridge. That would lick old Saussure into fits. All the Zermatt guides put the S. Theodul pass far beneath the Weissthör in point of difficulty; and you may tell Mrs. Hooker that they think the S. Theodul easier than the Monte Moro. The best of the joke was that I lost my way in coming down the Riffelberg to Zermatt the same evening, so that altogether I had a long day of it. The next day I walked from Zermatt to Visp (recovering Baedeker by the way), but my shoes were so knocked to pieces that I got a blister on my heel. Next day Voiture to Susten, and then over Gemmi to Kandersteg, and on Thursday my foot was so queer I was glad to get a *retour* to Interlaken. I found most interesting and complete evidences of old moraine deposits all the way down the Leuk valley into the Rhône valley, and I believe those little hills beyond Susten are old terminal moraines too. On the other side I followed moraines down to Frutigen, and great masses of glacial gravel with boulders, nearly to the Lake of Thun.

My wife is better, but anything but strong.

CHAMOUNIX, *Aug.* 16, 1857.

My wife sends me intelligence of the good news you were so kind as to communicate to her. I need not tell you how rejoiced I am that everything has gone on well, and that your

on its accustomed scale of magnificence, but I don't mind letting you know I am in the flesh and safe back.

The tour round Mont Blanc was a decided success; in fact, I had only to regret you were not with me. The grand glacier of the Allée Blanche and the view of Mont Blanc from the valley of Aosta were alone worth all the trouble. I had only one wet day, and that I spent on the Brenoa Glacier; for, in spite of all good resolutions to the contrary, I cannot resist poking into the glaciers whenever I have a chance. You will be interested in my results, which we shall soon, I hope, talk on together at length.

As I suspected, Forbes has made a most egregious blunder. What he speaks of and figures as the "structure" of the Brenon is nothing but a peculiar arrangement of *entirely superficial dirt bands, dependent on the structure, but not it*. The true structure is singularly beautiful and well marked in the Brenoa, the blue veins being very close set, and of course wholly invisible from a distance of a hundred yards, which is less than that of the spot whence Forbes' view of the (supposed) structure is taken.

I saw another wonderful thing in La Brenoa. About the middle of its length there is a step like this of about 20 or 30 feet in height. In the lower part (B) the structural planes are vertical; in the upper (A) they dip at a considerable angle. I thought I had found a case of unconformability, indicating a slip of one portion of the glacier over another, but when I came to examine the intermediate region (X) carefully, I found the structural planes at every intermediate angle, and consequently a perfect transition from the one to the other.

I returned by Aosta, the great St. Bernard, and the Col de Balme. Old Simond was quite affectionate in his discourse about you, and seemed quite unhappy because you would not borrow his money. He had received your remittance, and asked me to tell you so. He was distressed at having forgotten to get a certificate from you, so I said in mine I was quite sure you were well satisfied with him.

On our journey he displayed his characteristic qualities, *Je ne sais pas* being the usual answer to any topographical inquiries with a total absence of nerve, and a general conviction that distances were very great and that the weather would be bad. However, we got on very well, and I was sorry to part with him.

I came home by way of Neuchatel, paying a visit to the *Pierre à Bôt*, which I have long wished to see. My financial

Explanatory Catalogue to serve as an introductory textbook to the Jermyn Street lectures and the paleontological demonstrations. Here, too, would fall a proposed "Letter on the Study of Comparative Anatomy," to do for those subjects what Henslow had done in his "Letter" for Botany.

In addition to the fact of his being forced to take up Paleontology, it was perhaps the philosophic breadth of view with which he regarded his subject at any time, and the desire of getting to the bottom of each subsidiary problem arising from it, that made him for many years seem constantly to spring aside from his own subject, to fly off at a tangent from the line in which he was assured of unrivalled success did he but devote to it his undivided powers. But he was prepared to endure the charge of desultoriness with equanimity. In part, he was still studying the whole field of biological science before he would claim to be a master in one department; in part, he could not yet tell to what post he might succeed when he left—as he fully expected to leave—the professorship at Jermyn Street.

One characteristic of his early papers should not pass unnoticed. This was his familiarity with the best that had been written on his subjects abroad as well as in England. Thoroughness in this respect was rendered easier by the fact that he read French and German with almost as much facility as his mother tongue. "It is true, of course, that scientific men read French and German before the time of Huxley; but the deliberate consultation of all the authorities available has been maintained in historical succession since Huxley's earliest papers, and was absent in the papers of his early contemporaries." *

About this time his activity in several branches of science began to find recognition from scientific societies at home and abroad. In 1857 he was elected honorary member of the Microscopical Society of Giessen; and in the same year, of a more important body, the Academy of

* P. Chalmers Mitchell in *Natural Science*, August 1895.



came in at the head of the poll; no other having, *i.e.* Cobden, more than eleven.—Yours well satisfied,

ROD. I. MURCHISON.

From this time forth he corresponded with many foreign men of science; in these years particularly with Victor Carus, Lacaze Duthiers, Kölliker, and de Quatrefages, in reference to their common interest in the study of the invertebrates.

At home, the year 1857 opened very brightly for Huxley with the birth of his first child, a son, on the eve of the New Year. A Christmas child, the boy was named Noel, and lived four happy years to be the very sunshine of home, the object of passionate devotion, whose sudden loss struck deeper and more ineffaceably than any other blow that befell Huxley during all his life.

As he sat alone that December night, in the little room that was his study in the house in Waverley Place, waiting for the event that was to bring him so much happiness and so much sorrow, he made a last entry in his journal, full of hope and resolution. In the blank space below follows a note of four years later, when "the ground seemed cut from under his feet," yet written with restraint and without bitterness.

December 31, 1856. . . . 1856-7-8 must still be "Lehrjahre" to complete training in principles of Histology, Morphology, Physiology, Zoology, and Geology by *Monographic Work* in each Department. 1860 will then see me well grounded and ready for any special pursuits in either of these branches.

It is impossible to map out beforehand how this must be done. I must seize opportunities as they come, at the risk of the reputation of desultoriness.

In 1860 I may fairly look forward to fifteen or twenty years "Meisterjahre," and with the comprehensive views my training will have given me, I think it will be possible in that time to give a new and healthier direction to all Biological Science.

To smite all humbugs, however big; to give a nobler tone to science; to set an example of abstinence from petty personal controversies, and of toleration for everything but lying; to be indifferent as to whether the work is recognised as mine or not, so long as it is done:—are these my aims? 1860 will show.

Willst du dir ein hübsch Leben zimmern,
 Musst dich ans Vergangene nicht bekümmern ;
 Und wäre dir auch was Verloren,
 Musst immer thun wie neugeboren.
 Was jeder Tag will, sollst du fragen ;
 Was jeder Tag will, wird er sagen.
 Musst dich an eigenem Thun ergötzen ;
 Was andere thun, das wirst du schätzen.
 Besonders keinen Menschen hassen
 Und das Übrige Gott überlassen.*

Half-past ten at night.

Waiting for my child. I seem to fancy it the pledge that all these things shall be.

Born five minutes before twelve. Thank God. New Year's Day, 1857.

Sept. 20, 1860.

And the same child, our Noel, our first-born, after being for nearly four years our delight and our joy, was carried off by scarlet fever in forty-eight hours. This day week he and I had a great romp together. On Friday his restless head, with its bright blue eyes and tangled golden hair, tossed all day upon his pillow. On Saturday night the fifteenth, I carried him here into my study, and laid his cold still body here where I write. Here too on Sunday night came his mother and I to that holy leave-taking.

My boy is gone, but in a higher and a better sense than was in my mind when I wrote four years ago what stands above—I feel that my fancy has been fulfilled. I say heartily and without bitterness—Amen, so let it be.

* Wilt shape a noble life ? Then cast
 No backward glances to the past.
 And what if something still be lost ?
 Act as new-born in all thou dost.
 What each day wills, that shalt thou ask ;
 Each day will tell its proper task ;
 What others do, that shalt thou prize,
 In thine own work thy guerdon lies.
 This above all : hate none. The rest—
 Leave it to God. He knoweth best.

CHAPTER XII

1859-1860

THE programme laid down in 1857 was steadily carried out through a great part of 1859. Huxley published nine monographs, chiefly on fossil Reptilia, in the proceedings of the Geological Society and of the Geological Survey, one on the armour of crocodiles at the Linnean, and "Observations on the Development of some Parts of the Skeleton of Fishes," in the *Journal of Microscopical Science*.

Among the former was a paper on Stagonolepis, a creature from the Elgin beds, which had previously been ranked among the fishes. From some new remains, which he worked out of the stone with his own hands, Huxley made out that this was a reptile closely allied to the Crocodiles; and from this and the affinities of another fossil, Hyperodapedon, from neighbouring beds, determined the geological age to which the Elgin beds belonged. A good deal turned upon the nature of the scales from the back and belly of this animal, and a careful comparison with the scales of modern crocodiles—a subject till then little investigated—led to the paper at the Linnean already mentioned.

The paper on fish-development was mainly based upon dissections of the young of the stickleback. Fishes had been divided into two classes according as their tails are developed evenly on either side of the line of the spine, which was supposed to continue straight through the centre of the tail (homocercal), or lopsided, with one tail fin larger than the other (heterocercal). This investigation showed that the apparently even development was only an extreme case of lopsidedness,

The more I see of the place the more glad I am that I elected to stay in London. I see much to admire and like; but I am more and more convinced that it would not suit me as a residence.

Two more important points remain to be mentioned among the occupations of the year. In January Huxley was elected Secretary of the Geological Society, and with this office began a form of administrative work in the scientific world which ceased only with his resignation of the Presidency of the Royal Society in 1885.

Part of the summer Huxley spent in the North. On August 3 he went to Lamlash Bay in Arran. Here Dr. Carpenter had, in 1855, discovered a convenient cottage on Holy Island—the only one, indeed, on the island—well suited for naturalists; the bay was calm and suitable both for the dredge and for keeping up a vivarium. He proposed that either the Survey should rent the whole island at a cost of some £50, or, failing this, that he would take the cottage himself, if Huxley would join him for two or three seasons and share the expense. Huxley laid the plan before Sir R. Murchison, the head of the Survey, who consented to try the plan for a course of years, during three months in each year. “But,” he added, “keep it experimental; for there are no *useful* fisheries such as delight Lord Stanley.” Here, then, with an ascent of Goatfell for variety on the 21st, a month was passed in trawling, and experiments on the spawning of the herring appear to have been continued for him during the winter in Bute.

On the 29th Huxley left Lamlash for a trip through central and southern Scotland, continuing his geological work for the Survey; and wound up by attending the meeting of the British Association at Aberdeen, leaving his wife and the three children at Aberdour, on the Fifeshire coast.

From Aberdeen, where Prince Albert was President of the Association, Huxley writes on September 15:—

Owen’s brief address on giving up the presidential chair was exceedingly good. . . . I shall be worked like a horse here. There are all sorts of new materials from Elgin, besides other things, and I daresay I shall have to speak frequently. In point

Can't you come up this way as you go to Aberdeen?—Ever
yours faithfully,

T. H. HUXLEY.

P.S.—I thought I might mention the Jermyn Street matter to Faraday privately, and did so. He seemed pleased that the offer had been made.

The acceptance of the lectureship at the School of Mines brought Tyndall into the closest contact with Huxley for the next nine years, until he resigned his lectureship in 1868 on succeeding Faraday as superintendent of the Royal Institution.

On September 17 he writes:—

Yesterday Owen and I foregathered in Section D. He read a very good and important paper, and I got up afterwards and spoke exactly as I thought about it, and praising many parts of it strongly. In his reply he was unco civil and complimentary, so that the people who had come in hopes of a row were (as I intended they should be) disappointed.

A number of miscellaneous letters of this period are here grouped together.

14 WAVERLEY PLACE, *Jan.* 30, 1858.

MY DEAR HOOKER— . . . I wish you wouldn't be apologetic about criticism from people who have a right to criticise. I always look upon any criticism as a compliment, not but what the old Adam in T. H. H. *will* arise and fight vigorously against all impugnement, and irrespective of all odds in the way of authority, but that is the way of the beast.

Why I value your and Tyndall's and Darwin's friendship so much is, among other things, that you all pitch into me when necessary. You may depend upon it, however blue I may look when in the wrong, it's wrath with myself and nobody else.

TO HIS SISTER

THE GOVERNMENT SCHOOL OF MINES, JERMYN ST.,

March 27, 1858.

MY DEAREST LIZZIE—It is a month since your very welcome letter reached me. I had every inclination and every intention to answer it at once, but the wear and tear of incessant occupation (for your letter arrived in the midst of my busiest time) has, I will not say deprived me of the leisure, but of that tone of mind which one wants for writing a long letter. I fully

This is the account any third person would give you of what I am and of what I am doing. He would probably add that I was very ambitious and desirous of occupying a high place in the world's estimation. Therein, however, he would be mistaken. An income sufficient to place me above care and anxiety, and free scope to work, are the only things I have ever wished for or striven for. But one is obliged to toil long and hard for these, and it is only now that they are coming within my grasp. I gave up the idea of going to Edinburgh because I doubted whether leaving London was wise. Recently I have been tempted to put up for a good physiological chair which is to be established at Oxford; but the Government propose to improve my position at the School of Mines, and there is every probability that I shall now permanently remain in London. Indeed, it is high time that I should settle down to one line of work. Hitherto, as you see by the somewhat varied list of my duties, etc., above, I have been ranging over different parts of a very wide field. But this apparent desultoriness has been necessary, for I knew not for what branch of science I should eventually have to declare myself. There are very few appointments open to men of science in this country, and one must take what one can get and be thankful.

My health was very bad some years ago, and I had great fear of becoming a confirmed dyspeptic, but thanks to the pedestrian tours in the Alps I have taken for the past two years, I am wonderfully better this session, and feel capable of any amount of work. It was in the course of one of these trips that I went, as you have rightly heard, half way up Mont Blanc. But I was not in training and stuck at the Grands Mulets, while my three companions went on. I spent seventeen hours alone on that grand pinnacle, the latter part of the time in great anxiety, for I feared my friends were lost; and as I had no guide my own neck would have been in considerable jeopardy in endeavouring to return amidst the maze of crevasses of the Glacier des Bois. But it was glorious weather and the grandest scenery in the world. In the previous year I saw much of the Bernese and Monte Rosa country, journeying with a great friend of mine well known as a natural philosopher, Tyndall, and partly seeking health and partly exploring the glaciers. You will find an article of mine on that subject in the *Westminster Review* for 1857.

I used at one time to write a good deal for that Review, principally the Quarterly notice of scientific books. But I never write for the Reviews now, as original work is much more to

As you say, I have found this a great place for "work of price." I have finished the "Oceanic Hydrozoa" all but the bookwork, for which I must have access to the B.M. Library—but another week will do him. My notes are from eight to twelve years old, and really I often have felt like the editor of somebody else's posthumous work.

Just now I am busy over the "Croonian," which must be done before I return. I have been pulling at all the arguments as a spider does at his threads, and I think they are all strong. If so the thing will do some good.

I am perplexed about the N.H. Collections. The best thing, I firmly believe, would be for the Economic Zoology and a set of well selected types to go to Kensington, but I should be sorry to see the scientific collection placed under any such auspices as those which govern the "Bilers." I don't believe the clay soil of the Regent's Park would matter a fraction—and to have a grand scientific zoological and paleontological collection for working purposes close to the Gardens where the living beasts are, would be a grand thing. I should not wonder if the affair is greatly discussed at the B.A. at Leeds, and then, perhaps, light will arise.

Have you seen that madcap Tyndall's letter in the *Times*? He'll break his blessed neck some day, and that will be a great hole in the efficiency of my scientific young England. We mean to return next Saturday, and somewhere about the 16th or 17th I shall go down to York, where I want to study Plesiosaurs. I shall return after the British Association. The interesting question arises, Shall I have a row with the Great O. there? What a capital title that is they give him of the *British* Cuvier. He stands in exactly the same relation to the French as British brandy to cognac.—Ever yours faithfully, T. H. HUXLEY.

Am I to send the *Gardener's Chronicle* on, and where? please. I have mislaid the address.

JERMYN STREET, Oct. 25, 1858.

MY DEAR SPENCER—I read your article on the "Archetype" the other day with great delight, particularly the phrase which puts the Owenian and Cummingian interpolations on the same footing. It is rayther strong, but quite just.

I do not remember a word to object to, but I think I could have strengthened your argument in one or two places. Having eaten the food, will you let me have back the dish? I am wind-

When Nettie and I were young fools we agreed we would marry whenever we had £200 a year. Well, we have had more than twice that to begin upon, and how it is we have kept out of the Bench is a mystery to me. But we *have*, and I am inclined to think that the Missus has got a private hoard (out of the puddings) for Noel.

I shall leave Nettie to finish this rambling letter. In the meanwhile, my best love to you and yours, and mind you are a better correspondent than your affectionate brother, Tom.

TO PROFESSOR LEUCKART

THE GOVERNMENT SCHOOL OF MINES,
JERMYN STREET, LONDON, *January 30, 1859.*

MY DEAR SIR—Our mutual friend, Dr. Harley, informs me that you have expressed a wish to become possessed of a separate copy of my lectures, published in the *Medical Times*. I greatly regret that I have not one to send you. The publisher only gave me half a dozen separate copies of the numbers of the journal in which the Lectures appeared. Of these I sent one to Johannes Müller and one to Professor Victor Carus, and the rest went to other friends.

I am sorry to say that a mere fragment of what I originally intended to have published has appeared, the series having been concluded when I reached the end of the Crustacea. To say truth, the Lectures were not fitted for the journal in which they appeared.

I did not know that anyone in Germany had noticed them until I received the copy of your *Bericht* for 1856, which you were kind enough to send me. I owe you many thanks for the manner in which you speak of them, and I assure you it was a source of great pleasure and encouragement to me to find so competent a judge as yourself appreciating and sympathising with my objects.

Particular branches of zoology have been cultivated in this country with great success, as you are well aware, but ten years ago I do not believe that there were half a dozen of my countrymen who had the slightest comprehension of morphology, and of what you and I should call "Wissenschaftliche Zoologie."

Those who thought about the matter at all took Owen's osteological extravaganzas for the *ne plus ultra* of morphological speculation.

I learned the meaning of Morphology and the value of de-

get no money from the Government, and in the meanwhile you and Kölliker, Gegenbaur and Vogt, went to the shores of the Mediterranean and made sad havoc with my novelties. Then came occupations consequent on my appointment to the chair I now hold; and it was only last autumn that I had leisure to take up the subject again.

However, the plates, which I hope you will see in a few months have, with two exceptions, been engraved five years.

Pray make my remembrances to Dr. Eckhard. I was sorry not to have seen him again in London.—Ever, my dear Sir, very faithfully yours,

T. H. HUXLEY.

Prof. Leuckart.

At this time Sir J. Hooker was writing, as an introduction to his *Flora of Tasmania*, his essay on the *Flora of Australia*, published in 1859—a book which owed its form to the influence of Darwin, and in return lent weighty support to evolutionary theory from the botanical side. He sent his proofs for Huxley to read.

14 WAVERLEY PLACE, N.W., *April 22, 1859.*

MY DEAR HOOKER—I have read your proofs with a great deal of attention and interest. I was greatly struck with the suggestions in the first page, and the exposure of the fallacy “that cultivated forms recur to wild types if left alone” is new to me and seems of vast importance.

The argument brought forward in the note is very striking and as simple as the egg of Columbus, when one sees it. I have marked one or two passages which are not quite clear to me. . . .

I have been accused of writing papers composed of nothing but heads of chapters, and I think you tend the same way. Please take the trouble to make the two lines I have scored into a paragraph, so that poor devils who are not quite so well up in the subject as yourself may not have to rack their brains for an hour to supply all the links of your chain of argument. . . .

You see that I am in a carping humour, but the matter of the essays seems to me to be so very valuable that I am jealous of the manner of it.

I had a long visit from Greene of Cork yesterday. He is very Irish, but very intelligent and well-informed, and I am in hopes he will do good service. He is writing a little book on the Protozoa, which (so far as I have glanced over the proof

sheets as yet) seems to show a very philosophical turn of mind. It is very satisfactory to find the ideas one has been fighting for beginning to take root.

I do not suppose my own personal contributions to science will ever be anything very grand, but I shall be well content if I have reason to believe that I have done something to stir up others.—Ever yours faithfully,

T. H. HUXLEY.

To the same :—

April, 1859.

MY DEAR HOOKER . . . I pity you—as for the MSS. it is one of those cases for which penances were originally devised. What do you say to standing on your head in the garden for one hour per diem for the next week? It would be a relief. . . .

I suppose you will be at the Phil. Club next Monday. In the meanwhile don't let all the flesh be worried off your bones (there isn't much as it is).—Ever yours faithfully,

T. H. HUXLEY.

14 WAVERLEY PLACE, *July 29, 1859.*

MY DEAR HOOKER—I meant to have written to you yesterday, but things put it out of my head. If there is to be any fund raised at all, I am quite of your mind that it should be a scientific fund and not a mere naturalists' fund. Sectarianism in such matters is ridiculous, and besides that, in this particular case it is bad policy. For the word "Naturalist" unfortunately includes a far lower order of men than chemist, physicist, or mathematician. You don't call a man a mathematician because he has spent his life in getting as far as quadratics; but every fool who can make bad species and worse genera is a "Naturalist"!—save the mark! Imagine the chemists petitioning the Crown for a Pension for P—— if he wanted one! and yet he really is a philosopher compared to poor dear A——.

"Naturalists" therefore are far more likely to want help than any other class of scientific men, and they would be greatly damaging their own interests if they formed an exclusive fund for themselves.—Ever yours faithfully,

T. H. HUXLEY.

CHAPTER XIII

1859

IN November 1859 the *Origin of Species* was published, and a new direction was given to Huxley's activities. Ever since Darwin and Wallace had made their joint communication to the Linnean Society in the preceding July, expectation had been rife as to the forthcoming book. Huxley was one of the few privileged to learn Darwin's argument before it was given to the world; but the greatness of the book, mere instalment as it was of the long accumulated mass of notes, almost took him by surprise. Before this time, he had taken up a thoroughly agnostic attitude with regard to the species question, for he could not accept the creational theory, yet sought in vain among the transmutationists for any cause adequate to produce transmutation. He had had many talks with Darwin, and though ready enough to accept the main point, maintained such a critical attitude on many others, that Darwin was not by any means certain of the effect the published book would produce upon him. Indeed, in his 1857 notebook, I find jotted down under the head of his paper on *Pygocephalus* (read at the Geological Society), "anti-progressive confession of faith." Darwin was the more anxious, as, when he first put pen to paper, he had fixed in his mind three judges, by whose decision he determined mentally to abide. These three were Lyell, Hooker, and Huxley. If these three came round, partly through the book, partly through their own reflections, he could feel that the subject was safe. "No one," writes Darwin on November 13, "has read it, except Lyell, with whom I have had much correspondence.

is impossible. I confine myself to what must be regarded as a modest and reasonable request for some particle of evidence that the existing species of animals and plants did originate in that way, as a condition of my belief in a statement which appears to me to be highly improbable.

And, by way of being perfectly fair, I had exactly the same answer to give to the evolutionists of 1851-58. Within the ranks of the biologists, at that time, I met with nobody, except Dr. Grant of University College, who had a word to say for Evolution—and his advocacy was not calculated to advance the cause. Outside these ranks, the only person known to me whose knowledge and capacity compelled respect, and who was, at the same time, a thorough-going evolutionist, was Mr. Herbert Spencer, whose acquaintance I made, I think, in 1852, and then entered into the bonds of a friendship which, I am happy to think, has known no interruption. Many and prolonged were the battles we fought on this topic. But even my friend's rare dialectic skill and copiousness of apt illustration could not drive me from my agnostic position. I took my stand upon two grounds:—Firstly, that up to that time, the evidence in favour of transmutation was wholly insufficient; and secondly, that no suggestion respecting the causes of transmutation assumed, which had been made, was in any way adequate to explain the phenomena. Looking back at the state of knowledge at that time, I really do not see that any other conclusion was justifiable.

In those days I had never even heard of Treviranus' *Biologie*. However, I had studied Lamarck attentively, and I had read the *Vestiges* with due care; but neither of them afforded me any good ground for changing my negative and critical attitude. As for the *Vestiges*, I confess that the book simply irritated me by the prodigious ignorance and thoroughly unscientific habit of mind manifested by the writer. If it had any influence on me at all, it set me against Evolution; and the only review I ever have qualms of conscience about, on the ground of needless savagery, is one I wrote on the *Vestiges* while under that influence.* . . .

But, by a curious irony of fate, the same influence which led me to put as little faith in modern speculations on this subject as in the venerable traditions recorded in the first two chapters of Genesis, was perhaps more potent than any other in keeping alive a sort of pious conviction that Evolution, after all, would turn out true. I have recently read afresh the first edition of

* See Appendix.

transitional forms, with all the confidence of youth and imperfect knowledge. I was not aware, at that time, that he had then been many years brooding over the species-question; and the humorous smile which accompanied his gentle answer, that such was not altogether his view, long haunted and puzzled me. But it would seem that four or five years' hard work had enabled me to understand what it meant; for Lyell, writing to Sir Charles Bunbury (under date of April 30, 1856), says:—

“When Huxley, Hooker and Wollaston were at Darwin's last week, they (all four of them) ran a tilt against species—further, I believe, than they are prepared to go.”

I recollect nothing of this beyond the fact of meeting Mr. Wollaston; and except for Sir Charles's distinct assurance as to “all four,” I should have thought my *outré* was probably a counterblast to Wollaston's conservatism. With regard to Hooker, he was already, like Voltaire's Habbakuk, *capable de tout* in the way of advocating Evolution.

As I have already said, I imagine that most of those of my contemporaries who thought seriously about the matter, were very much in my own state of mind—inclined to say to both Mosaists and Evolutionists, “a plague on both your houses!” and disposed to turn aside from an interminable and apparently fruitless discussion, to labour in the fertile fields of ascertainable fact. And I may therefore suppose that the publication of the Darwin and Wallace paper in 1858, and still more that of the “Origin” in 1859, had the effect upon them of the flash of light which, to a man who has lost himself on a dark night, suddenly reveals a road which, whether it takes him straight home or not, certainly goes his way. That which we were looking for, and could not find, was a hypothesis respecting the origin of known organic forms which assumed the operation of no causes but such as could be proved to be actually at work. We wanted, not to pin our faith to that or any other speculation, but to get hold of clear and definite conceptions which could be brought face to face with facts and have their validity tested. The “Origin” provided us with the working hypothesis we sought. Moreover, it did the immense service of freeing us for ever from the dilemma—Refuse to accept the creation hypothesis, and what have you to propose that can be accepted by any cautious reasoner? In 1857 I had no answer ready, and I do not think that anyone else had. A year later we reproached ourselves with dulness for being perplexed with such an inquiry. My reflection, when I first made myself master of

from letters . . . and from remarks, the most serious omission in my book was not explaining how it is, as I believe, that all forms do not necessarily advance, how there can now be *simple* organisms existing." (May 22, 1860.)

Huxley's idea, then, was to call attention to the persistence of many types without appreciable progression during geological time; to show that this fact was not explicable on any other hypothesis than that put forward by Darwin; and by paleontological arguments, to pave the way for consideration of the imperfection of the geological record.

Such were the lines on which he delivered his Friday evening lecture on "Persistent Types" at the Royal Institution on June 3, 1859.

However, the chief part which he took at this time in extending the doctrines of evolution was in applying them to his own subjects, Development and Vertebrate Anatomy, and more particularly to the question of the origin of mankind.

Of all the burning questions connected with the Origin of Species, this was the most heated—the most surrounded by prejudice and passion. To touch it was to court attack; to be exposed to endless scorn, ridicule, misrepresentation, abuse—almost to social ostracism. But the facts were there; the structural likenesses between the apes and man had already been shown; and as Huxley warned Darwin, "I will stop at no point so long as clear reasoning will carry me further."

Now two years before the "Origin" appeared, the denial of these facts by a leading anatomist led Huxley, as was his wont, to re-investigate the question for himself and satisfy himself one way or the other. He found that the previous investigators were not mistaken. Without going out of his way to refute the mis-statement as publicly as it was made, he simply embodied his results in his regular teaching. But the opportunity came unsought. Fortified by his own researches, he openly challenged these assertions when repeated at the Oxford meeting of the British Association in 1860, and promised to make good his challenge in the proper place.

atom of one element for an atom of another element. You may in this way produce a vast series of modifications—but each modification is definite in its composition, and there are no transitional or intermediate steps between one definite compound and another. I have a sort of notion that similar laws of definite combination rule over the modifications of organic bodies, and that in passing from species to species “*Natura fecit saltum*.”

All my studies lead me to believe more and more in the absence of any real transitions between natural groups, great and small—but with what we know of the physiology of conditions [?] this opinion seems to me to be quite consistent with transmutation.

When I say that no evidence, or hardly any, would justify one in believing in the rise of a new species of Elephant, *e.g.* out of the earth, I mean that such an occurrence would be so diametrically contrary to all experience, so opposed to those beliefs which are the most constantly verified by experience, that one would be justified in believing either that one’s senses were deluded, or that one had not really got to the bottom of the phenomenon. Of course, if one could vary the conditions, if one could take a little silex, and by a little *hocus-pocus* à la crosse, galvanise a baby out of it as often as one pleased, all the philosopher could do would be to hold up his hands and cry, “God is great.” But short of evidence of this kind, I don’t mean to believe anything of the kind.

How much evidence would you require to believe that there was a time when stones fell upwards, or granite made itself by a spontaneous rearrangement of the elementary particles of clay and sand? And yet the difficulties in the way of these beliefs are as nothing compared to those which you would have to overcome in believing that complex organic beings made themselves (for that is what creation comes to in scientific language) out of inorganic matter.

I know it will be said that even on the transmutation theory, the first organic being must have made itself. But there is as much difference between supposing the passage of inorganic matter into an *amoeba*, *e.g.*, and into an *Elephant*, as there is between supposing that Portland stone might have built itself up into St. Paul’s, and believing that the Giant’s Causeway may have come about by natural causes.

True, one must believe in a beginning somewhere, but science consists in not believing the having reached that beginning before one is forced to do so.

It is wholly impossible to prove that any phenomenon whatsoever is not produced by the interposition of some unknown cause. But philosophy has prospered exactly as it has disregarded such possibilities, and has endeavoured to resolve every event by ordinary reasoning.

I do not exactly see the force of your argument that we are bound to find fossil forms intermediate between men and monkeys in the Rocks. Crocodiles are the highest reptiles as men are the highest mammals, but we find nothing intermediate between *crocodilia* and *lacertilia* in the whole range of the Mesozoic rocks. How do we know that Man is not a persistent type? And as for implements, at this day, and as, I suppose, for the last two or three thousand years at least, the savages of Australia have made their weapons of nothing but bone and wood. Why should *Homo Eocenus* or *Ooliticus*, the fellows who waddied the *Amphitherium* and speared the *Phascolotherium* as the Australian niggers treat their congeners, have been more advanced?

I by no means suppose that the transmutation hypothesis is proven or anything like it. But I view it as a powerful instrument of research. Follow it out, and it will lead us somewhere; while the other notion is like all the modifications of "final causation," a barren virgin.

And I would very strongly urge upon you that it is the logical development of Uniformitarianism, and that its adoption would harmonise the spirit of Paleontology with that of Physical Geology.

CHAPTER XIV

1859-60

THE "Origin" appeared in November. As soon as he had read it, Huxley wrote the following letter to Darwin (already published in *Life of Darwin*, vol. ii. p. 231):—

JERMYN STREET, W., *November 23, 1859.*

MY DEAR DARWIN—I finished your book yesterday, a lucky examination having furnished me with a few hours of continuous leisure.

Since I read Von Bär's essays, nine years ago, no work on Natural History Science I have met with has made so great an impression upon me, and I do most heartily thank you for the great store of new views you have given me. Nothing, I think, can be better than the tone of the book; it impresses those who know about the subject. As for your doctrine, I am prepared to go to the stake, if requisite, in support of Chapter IX * and most parts of Chapters X, XI, XII, and Chapter XIII contains much that is most admirable, but on one or two points I enter a *caveat* until I can see further into all sides of the question.

As to the first four chapters,† I agree thoroughly and fully with all the principles laid down in them. I think you have demonstrated a true cause for the production of species, and have thrown the *onus probandi*, that species did not arise in the way you suppose, on your adversaries.

* Chapter IX, The Imperfection of the Geological Record; X, The Geological Succession of Organic Beings; XI–XII, Geographical Distribution; XIII, Classification, Morphology, Embryology, and Rudimentary Organs.

† Chapter I, Variation under Domestication; II, Variation under Nature; III, The Struggle for Existence; IV, Operation of Natural Selection; V, Laws of Variation.

of giving the book a fair chance with the multitudinous readers of the *Times*, to make any difficulty about conditions; and being then very full of the subject, I wrote the article faster, I think, than I ever wrote anything in my life, and sent it to Mr. Lucas, who duly prefixed his opening sentences.

When the article appeared, there was much speculation as to its authorship. The secret leaked out in time, as all secrets will, but not by my aid; and then I used to derive a good deal of innocent amusement from the vehement assertions of some of my more acute friends, that they knew it was mine from the first paragraph!

As the *Times* some years since, referred to my connection with the review, I suppose there will be no breach of confidence in the publication of this little history, if you think it worth the space it will occupy.

The article appeared on December 26. Only Hooker was admitted into the secret. In an undated note Huxley writes to him:—

I have written the other review you wot of, and have handed it over to my friend to deal as he likes with it. . . . Darwin will laugh over a letter that I sent him this morning with a vignette of the Jermyn Street “pet” ready to fight his battle, and the “judicious Hooker” holding the bottle.

And on December 31 he writes again:—

JERMYN STREET, *December 31, 1859.*

MY DEAR HOOKER—I have not the least objection to my share in the *Times* article being known, only I should not like to have anything stated on my authority. The fact is, that the first quarter of the first column (down to “what is a species,” etc.) is not mine, but belongs to the man who is the official reviewer for the *Times* (my “Temporal” godfather I might call him).

The rest is in my *ipsissima verba*, and I only wonder that it turns out as well as it does—for I wrote it faster than ever I wrote anything in my life. The last column nearly as fast as my wife could read the sheets. But I was thoroughly in the humour and full of the subject. Of course as a scientific review the thing is worth nothing, but I earnestly hope it may have made some of the educated mob, who derive their ideas from the *Times*, reflect. And whatever they do, they *shall* respect Darwin.

Pray give my kindest regards and best wishes for the New

So "Dr. Draper droned out his paper, turning first to the right hand and then to the left, of course bringing in a reference to the Origin of Species which set the ball rolling."

An hour or more that paper lasted, and then discussion began. The President "wisely announced *in limine* that none who had not valid arguments to bring forward on one side or the other would be allowed to address the meeting; a caution that proved necessary, for no fewer than four combatants had their utterances burked by him, because of their indulgence in vague declamation." *

First spoke (writes Professor Farrar †) a layman from Brompton, who gave his name as being one of the Committee of the (newly formed) Economic section of the Association. He, in a stentorian voice, let off his theological venom. Then jumped up Richard Greswell ‡ with a thin voice, saying much the same, but speaking as a scholar; but we did not merely want any theological discussion, so we shouted them down. Then a Mr. Dingle got up and tried to show that Darwin would have done much better if he had taken him into consultation. He used the blackboard and began a mathematical demonstration on the question—"Let this point A be man, and let that point B be the mawnkey." He got no further; he was shouted down with cries of "mawnkey." None of these had spoken more than three minutes. It was when these were shouted down that Henslow said he must demand that the discussion should rest on *scientific* grounds only.

Then there were calls for the Bishop, but he rose and said he understood his friend Professor Beale had something to say first. Beale, who was an excellent histologist, spoke to the effect that the new theory ought to meet with fair discussion, but added, with great modesty, that he himself had not sufficient knowledge to discuss the subject adequately. Then the Bishop spoke the speech that you know, and the question about his mother being an ape, or his grandmother.

From the scientific point of view, the speech was of small value. It was evident from his mode of handling the subject that he had been "crammed up to the throat," and

* *Life of Darwin, l.c.*

† Canon of Durham.

‡ Rev. Richard Greswell, B.D., Tutor of Worcester College.

did not dawn upon Sir Benjamin until after Huxley had completed his "forcible and eloquent" answer to the scientific part of the Bishop's argument, and proceeded to make his famous retort.*

On this (continues the writer in *Macmillan's Magazine*) Mr. Huxley slowly and deliberately arose. A slight tall figure, stern and pale, very quiet and very grave,† he stood before us and spoke those tremendous words—words which no one seems sure of now, nor, I think, could remember just after they were spoken, for their meaning took away our breath, though it left us in no doubt as to what it was. He was not ashamed to have a monkey for his ancestor; but he would be ashamed to be connected with a man who used great gifts to obscure the truth. No one doubted his meaning, and the effect was tremendous. One lady fainted and had to be carried out; I, for one, jumped out of my seat.

The fullest and probably most accurate account of these concluding words is the following, from a letter of the late

* The *Athenæum* reports him as saying that Darwin's theory was an explanation of phenomena in Natural History, as the undulatory theory was of the phenomena of light. No one objected to that theory because an undulation of light had never been arrested and measured. Darwin's theory was an explanation of facts, and his book was full of new facts, all bearing on his theory. Without asserting that every part of that theory had been confirmed, he maintained that it was the best explanation of the origin of species which had yet been offered. With regard to the psychological distinction between men and animals, man himself was once a monad—a mere atom, and nobody could say at what moment in the history of his development he became consciously intelligent. The question was not so much one of a transmutation or transition of species, as of the production of forms which became permanent.

Thus the short-legged sheep of America was not produced gradually, but originated in the birth of an original parent of the whole stock, which had been kept up by a rigid system of artificial selection.

† "Young, cool, quiet, scientific—scientific in fact and in treatment."—J. R. Green. A certain piquancy must have been added to the situation by the superficial resemblance in feature between the two men, so different in temperament and expression. Indeed next day at Hardwicke, a friend came up to Mr. Fanning and asked who his guest was, saying, "Surely it is the son of the Bishop of Oxford."

No doubt your father's words were better than these, and they gained effect from his clear, deliberate utterance, but in outline and in *scale* this represents truly what was said.

After the commotion was over, "some voices called for Hooker, and his name having been handed up, the President invited him to give his view of the theory from the Botanical side. This he did, demonstrating that the Bishop, by his own showing, had never grasped the principles of the 'Origin,' and that he was absolutely ignorant of the elements of botanical science. The Bishop made no reply, and the meeting broke up." *

ACCOUNT OF THE OXFORD MEETING by the REV. W. H. FREEMANTLE (in *Charles Darwin, his Life Told, &c.*, 1892, p. 238).

The Bishop of Oxford attacked Darwin, at first playfully, but at last in grim earnest. It was known that the Bishop had written an article against Darwin in the last *Quarterly Review*; † it was also rumoured that Professor Owen had been staying at Cuddesdon and had primed the Bishop, who was to act as mouthpiece to the great Palæontologist, who did not himself dare to enter the lists. The Bishop, however, did not show himself master of the facts, and made one serious blunder. A fact which had been much dwelt on as confirmatory of Darwin's idea of variation, was that a sheep had been born shortly before in a flock in the North of England, having an addition of one to the vertebræ of the spine. The Bishop was declaring with rhetorical exaggeration that there was hardly any evidence on Darwin's side. "What have they to bring forward?" he exclaimed. "Some rumoured statement about a long-legged sheep." But he passed on to banter: "I should like to ask Professor Huxley, who is sitting by me, and is about to tear me to pieces when I have sat down, as to his belief in being descended from an ape. Is it on his grandfather's or his grandmother's side that the ape ancestry comes in?" And then taking a graver tone, he asserted, in a solemn peroration, that Darwin's views were contrary to the revelation of God in the Scriptures. Professor Huxley was unwilling to respond: but

* *Life of Darwin, l.c.*

† It appeared in the ensuing number for July.

The odd part of the business is, that I should not have been present except for Robert Chambers. I had heard of the Bishop's intention to utilise the occasion. I knew he had the reputation of being a first-class controversialist, and I was quite aware that if he played his cards properly, we should have little chance, with such an audience, of making an efficient defence. Moreover, I was very tired, and wanted to join my wife at her brother-in-law's country house near Reading, on the Saturday. On the Friday I met Chambers in the street, and in reply to some remark of his, about his going to the meeting, I said that I did not mean to attend it—did not see the good of giving up peace and quietness to be episcopally pounded. Chambers broke out into vehement remonstrances, and talked about my deserting them. So I said, "Oh! if you are going to take it that way, I'll come and have my share of what is going on."

So I came, and chanced to sit near old Sir Benjamin Brodie. The Bishop began his speech, and to my astonishment very soon showed that he was so ignorant that he did not know how to manage his own case. My spirits rose proportionately, and when he turned to me with his insolent question, I said to Sir Benjamin, in an undertone, "The Lord hath delivered him into mine hands."

That sagacious old gentleman stared at me as if I had lost my senses. But, in fact, the Bishop had justified the severest retort I could devise, and I made up my mind to let him have it. I was careful, however, not to rise to reply, until the meeting called for me—then I let myself go.

In justice to the Bishop, I am bound to say he bore no malice, but was always courtesy itself when we occasionally met in after years. Hooker and I walked away from the meeting together, and I remember saying to him that this experience had changed my opinion as to the practical value of the art of public speaking, and that from that time forth I should carefully cultivate it, and try to leave off hating it. I did the former, but never quite succeeded in the latter effort.

I did not mean to trouble you with such a long scrawl when I began about this piece of ancient history.—Ever yours very faithfully,

T. H. HUXLEY.

In the evening there was a crowded conversazione in Dr. Daubeney's rooms, and here, continues the writer in *Macmillan's*, "everyone was eager to congratulate the hero of the day. I remember that some naive person wished 'it

the applause grew and widened, until, when he sat down, the cheering was not very much less than that given to the Bishop. To that extent he carried an unwilling audience with him by the force of his speech. The debate on the ape question, however, was continued elsewhere during the next two years, and the evidence was completed by the unanswerable demonstrations of Sir W. H. Flower at the Cambridge meeting of the Association in 1862.

The importance of the Oxford meeting lay in the open resistance that was made to authority, at a moment when even a drawn battle was hardly less effectual than acknowledged victory. Instead of being crushed under ridicule, the new theories secured a hearing, all the wider, indeed, for the startling nature of their defence.

at the Royal Institution (February 8) on "The Nature of the Earliest Stages of Development of Animals."

Meanwhile the publication of these researches led to another pitched battle, in which public interest was profoundly engaged. The controversy which raged had some resemblance to a duel over a point of honour and credit. Scientific technicalities became the catchwords of society, and the echoes of the great Hippocampus question linger in the delightful pages of the *Water-Babics*. Of this fight Huxley writes to Sir J. Hooker on April 18, 1861:—

A controversy between Owen and myself, which I can only call absurd (as there is no doubt whatever about the facts), has been going on in the *Athenæum*, and I wound it up in disgust last week.

And again on April 27:—

Owen occupied an entirely untenable position—but I am nevertheless surprised he did not try "abusing plaintiff's attorney." The fact is he made a prodigious blunder in commencing the attack, and now his only chance is to be silent and let people forget the exposure. I do not believe that in the whole history of science there is a case of any man of reputation getting himself into such a contemptible position. He will be the laughing-stock of all the continental anatomists.

Rolleston has a great deal of Oxford slough to shed, but on that very ground his testimony has been of most especial service. Fancy that man — telling Maskelyne that Rolleston's observations were entirely confirmatory of Owen.

About the same time he writes to his wife:—

April 16.—People are talking a good deal about the "Man and the Apes" question, and I hear that somebody, I suspect Monckton Milnes, has set afloat a poetical squib on the subject.* . . .

* The squib in question, dated "the Zoological Gardens," and signed "Gorilla," appeared in *Punch* for May 15, 1861, under a picture of that animal, bearing the sign, "*Am I a Man and a Brother?*"

The concluding verses run as follows:

Next *HUXLEY* replies
That *OWEN* he lies
And garbles his Latin quotation;
That his facts are not new,
His mistakes not a few,
Detrimental to his reputation.

"To twice slay the slain"
By dint of the Brain
(Thus *HUXLEY* concludes his review),
Is but labour in vain,
Unproductive of gain,
And so I shall bid you "Adieu!"

motions," he steadfastly refused to be an advocate of the theory, "if by an advocate is meant one whose business it is to smooth over real difficulties, and to persuade when he cannot convince."

In common fairness he warned his audience of the one missing link in the chain of evidence—the fact that selective breeding has not yet produced species sterile to one another. But it is to be adopted as a working hypothesis like other scientific generalisations, "subject to the production of proof that physiological species may be produced by selective breeding; just as a physical philosopher may accept the undulatory theory of light, subject to the proof of the existence of the hypothetical ether; or as the chemist adopts the atomic theory, subject to the proof of the existence of atoms; and for exactly the same reasons, namely, that it has an immense amount of *prima facie* probability; that it is the only means at present within reach of reducing the chaos of observed facts to order; and lastly, that it is the most powerful instrument of investigation which has been presented to naturalists since the invention of the natural system of classification, and the commencement of the systematic study of embryology."

As for the repugnance of most men to admitting kinship with the apes, "thoughtful men," he says, "once escaped from the blinding influences of traditional prejudices, will find in the lowly stock whence man has sprung the best evidence of the splendour of his capacities; and will discern, in his long progress through the past, a reasonable ground of faith in his attainment of a nobler future."

A simile, with which he enforced this elevating point of view, which has since eased the passage of many minds to the acceptance of evolution, seems to have been much appreciated by his audience. It was a comparison of man to the Alps, which turn out to be "of one substance with the dullest clay, but raised by inward forces to that place of proud and seemingly inaccessible glory."

The lectures were met at first with astonishing quiet, but it was not long before the stones began to fly. The *Witness* of January 11 lashed itself into a fury over the fact

JERMYN STREET, *January 16, 1862.*

MY DEAR HOOKER—I wonder if we are ever to meet again in this world! At anyrate I send to the remote province of Kew, Greeting, and my best wishes for the new year to you and yours. I also inclose a slip from an Edinburgh paper containing a report of my lecture on the "Relation of Man," etc. As you will see, I went in for the entire animal more strongly, in fact, than they have reported me. I told them in so many words that I entertained no doubt of the origin of man from the same stock as the apes.

And to my great delight, in saintly Edinburgh itself the announcement met with nothing but applause. For myself I can't say that the praise or blame of my audience was much matter, but it is a grand indication of the general disintegration of old prejudices which is going on.

I shall see if I cannot make something more of the lectures by delivering them again in London, and then I shall publish them.

The report does not put nearly strong enough what I said in favour of Darwin's views. I affirmed it to be the only scientific hypothesis of the origin of species in existence, and expressed my belief that the one gap in the evidence would be filled up, as I always do.—Ever yours faithfully,

T. H. HUXLEY.

JERMYN STREET, *January 20, 1862.*

MY DEAR DARWIN—The inclosed article, which has been followed up by another more violent, more scurrilously personal, and more foolish, will prove to you that my labour has not been in vain, and that your views and mine are likely to be better ventilated in Scotland than they have been.

I was quite uneasy at getting no attack from the *Witness*, thinking I must have overestimated the impression I had made, and the favourableness of the reception of what I said. But the raving of the *Witness* is clear testimony that my notion was correct.

I shall send a short reply to the *Scotsman* for the purpose of further advertising the question.

With regard to what are especially your doctrines, I spoke much more favourably than I am reported to have done. I expressed no doubt as to their ultimate establishment, but as I particularly wished not to be misrepresented as an advocate trying to soften or explain away real difficulties, I did not in speaking enter into the details of what is to be said in diminish-

ment of the three objects of his address, namely, to state fully and fairly his conclusions, to avoid giving unnecessary offence, and thirdly, "while feeling assured of the just and reasonable dealing of the respectable part of the Scottish press, I naturally hoped for noisy injustice and unreason from the rest, seeing, as I did, the best security for the dissemination of my views through regions which they might not otherwise reach, in the certainty of a violent attack by (the *Witness*)."

The applause of the audience, he says, afforded him genuine satisfaction, "because it bids me continue in the faith on which I acted, that a man who speaks out honestly and fearlessly that which he knows, and that which he believes, will always enlist the good-will and the respect, however much he may fail in winning the assent, of his fellow-men."

About this time a new field of interest was opened out to him, closely connected with, indeed, and completing, the ape question. Sir Charles Lyell was engaged in writing his *Antiquity of Man*, and asked Huxley to supply him with various anatomical data touching the ape question, and later to draw him a diagram illustrating the peculiarities of the newly discovered Neanderthal skull as compared with other skulls. He points out in his letters to Lyell that the range of cranial capacity between the highest and the lowest German—"one of the mediatised princes, I suppose" *—or the Malayan or Peruvian, is almost 100 per cent; in absolute amount twice as much as the difference between that of the largest simian and the smallest human capacity, so that in seeking an ordinal difference between man and the apes, "it would certainly be well to let go the head, though I am afraid it does not mend matters much to lay hold of the foot."

And on January 25, 1862:—

I have been skull-measuring all day at the College of Surgeons. The *Neanderthal skull* may be described as a slightly

* The minor princes of Germany, whose territories were annexed to larger states, and who thus exchanged a direct for a mediate share in the imperial government.

an association meeting the other day. We all missed you, but I think it was as well you did not come, for though I am pretty tough, as you know, I found the pace rather killing. Nothing could exceed the hospitality and kindness of the University people—and that, together with a great deal of speaking on the top of a very bad cold, which I contrived to catch just before going down, has somewhat used me up.

Owen came down with the obvious intention of attacking me on all points. Each of his papers was an attack, and he went so far as to offer stupid and unnecessary opposition to proposals of mine in my own committee. However, he got himself sold at all points. . . . The Polypterus paper and the Aye-Aye paper fell flat. The latter was meant to raise a discussion on your views, but it was all a stale hash, and I only made some half sarcastic remarks which stopped any further attempts at discussion. . . .

I took my book to Scotland but did nothing. I shall ask leave to send you a bit or two as I get on.—Ever yours,

T. H. HUXLEY.

A “Society for the propagation of common honesty in all parts of the world” was established at Cambridge. I want you to belong to it, but I will say more about it by and by.

This admirable society, which was also to “search for scientific truth, especially in biology,” seems to have been but short lived. At all events, I can find only two references to subsequent meetings, on October 7 and December 19 in this year.

A few days later a final blow was struck in the battle over the ape question. He writes on October 15 how he has written a letter to the *Medical Times*—his last word on the subject, summing up in most emphatic terms:—

I have written the letter with the greatest care, and there is nothing coarse or violent in it. But it shall put an end to all the humbug that has been going on. . . . Rolleston will come out with his letter in the same number, and the smash will be awful, but most thoroughly merited.

These several pieces of work, struck out at different times in response to various impulses, were now combined and re-shaped into *Man's Place in Nature*, the first book which was published by him. Thus he writes to Sir Charles Lyell on May 5, 1862:—

win will, I think, take his place with such men as Harvey, and even if he is wrong his sobriety and accuracy of thought will put him on a far different level from Lamarck. I want to make this clear to people.

I am disposed to agree with you about the "emasculate" and "uncircumcised"—partly for your reasons, partly because I believe it is an excellent rule always to erase anything that strikes one as particularly smart when writing it. But it is a great piece of self-denial to abstain from expressing my peculiar antipathy to the people indicated, and I hope I shall be rewarded for the virtue.

As to the secondary causes I only wished to guard myself from being understood to imply that I had any comprehension of the meaning of the term. If my phrase looks naughty I will alter it. What I want is to be read, and therefore to give no unnecessary handle to the enemy. There will be row enough whatever I do.

Our Commission here * implicates us in an inquiry of some difficulty, and which involves the interests of a great many poor people. I am afraid it will not leave me very much leisure. But we are in the midst of a charming country, and the work is not unpleasant or uninteresting. If the sun would only shine more than once a week it would be perfect. — With kind remembrances to Lady Lyell, believe me, faithfully yours,

T. H. HUXLEY.

We shall be here for the next ten days at least. But my wife will always know my whereabouts.

JERMYN STREET, *March 23*, 1863.

MY DEAR SIR CHARLES—I suspect that the passage to which you refer must have been taken from my unrevised proofs, for it corresponds very nearly with what is written at p. 97 of my book.

Flower has recently discovered that the Siamang's brain affords an even more curious exception to the general rule than that of *Mycetes*, as the cerebral hemispheres leave part not only of the sides but of the hinder end of the cerebellum uncovered.

As it is one of the Anthropoid apes and yet differs in this respect far more widely from the gorilla than the gorilla differs from man, it offers a charming example of the value of cerebral characters.

* The Fishery Commission.

opinion of Mansel's Bampton Lectures on the *Limits of Religious Thought*:—

A friend of mine, Huxley, who will soon take rank as one of the first naturalists we have ever produced, begged me to read these sermons as first rate, "although, regarding the author as a churchman, you will probably compare him, as I did, to the drunken fellow in Hogarth's contested election, who is sawing through the signpost at the other party's public-house, forgetting he is sitting at the other end of it. But read them as a piece of clear and unanswerable reasoning."

In the 1894 preface to the re-issue of *Man's Place* in the Collected Essays, Huxley speaks as follows of the warnings he received against publishing on so dangerous a topic, of the storm which broke upon his head, and the small result which, in the long run, it produced * :

Magna est veritas et prævalebit! Truth is great, certainly, but considering her greatness, it is curious what a long time she is apt to take about prevailing. When, towards the end of 1862, I had finished writing *Man's Place in Nature*, I could say with a good conscience that my conclusions "had not been formed hastily or enunciated crudely." I thought I had earned the right to publish them, and even fancied I might be thanked rather than reproved for doing so. However, in my anxiety to publish nothing erroneous, I asked a highly competent anatomist and very good friend of mine to look through my proofs, and, if he could, point out any errors of fact. I was well pleased when he returned them without criticism on that score; but my satisfaction was speedily dashed by the very earnest warning as to the consequences of publication, which my friend's interest in my welfare led him to give. But, as I have confessed elsewhere, when I was a young man, there was just a little—a mere *soupçon*—in my composition of that tenacity of purpose which has another name; and I felt sure that all the evil things prophesied would not be so painful to me as the giving up that which I had resolved to do, upon grounds which I conceived to be right.*

* In September 1887 he wrote to Mr. Edward Clodd—"All the propositions laid down in the wicked book, which was so well anathematised a quarter of a century ago, are now taught in the text-books. What a droll world it is!"

† As to this advice not to publish *Man's Place* for fear of misrepresentation on the score of morals, he said, in criticising an attack of

—that I picked up “Atavism” in Pritchard years ago, and as it is a much more convenient word than “Hereditary transmission of variations,” it slipped into equivalence in my mind, and I forgot all about the original limitation.

But if these excuses should in your judgment tend to aggravate my offences, suppress 'em like a friend. One may always hope more from a lady's tender-heartedness than from her sense of justice.

Publisher has just sent to say that I must give him any corrections for second thousand of my booklet immediately.

Why did not Miss Etty send any critical remarks on that subject by the same post? I should be most immensely obliged for them.—Ever yours faithfully,

T. H. HUXLEY.

During this period of special work at the anthropological side of the Evolution theory, Huxley made two important contributions to the general question.

As secretary of the Geological Society, the duty of delivering the anniversary address in 1862 fell to him in the absence of the president, Leonard Horner, who had been driven by ill-health to winter in Italy.

The object at which he aimed appears from the postscript of a brief note of Feb. 19, 1862, to Hooker:—

I am writing the body of the address, and I am going to criticise Palæontological doctrines in general in a way that will flutter their nerves considerable.

Darwin is met everywhere with — Oh this is opposed to palæontology, or that is opposed to palæontology—and I mean to turn round and ask, “Now, Messieurs les Palæontologues, what the devil *do* you really know?”

I have not changed sex, although the postscript is longer than the letter.

The delivery of the address * itself on February 21 is thus described by Sir Charles Lyell † (*Life and Letters*, ii. 356):—

Huxley delivered a brilliant critical discourse on what palæontology has and has not done, and proved the value of

* On “Geological Contemporaneity” (*Coll. Ess.* viii. 292).

† To a note of whose, proposing a talk over the subject, Huxley replies on May 5, “I am very glad you find something to think about in my address. That is the best of all praise.”

Since I saw you—indeed, from the following Tuesday onwards—I have amused myself by spending ten days or so in bed. I had an unaccountable prostration of strength which they called influenza, but which, I believe, was nothing but some obstruction in the liver.

Of course I can't persuade people of this, and they will have it that it is overwork. I have come to the conviction, however, that steady work hurts nobody, the real destroyer of hardworking men being not their work, but dinners, late hours, and the universal humbug and excitement of society.

I mean to get out of all that and keep out of it.—Ever yours
faithfully, T. H. HUXLEY.

The other contribution to the general question was his Working Men's Lectures for 1862. As he writes to Darwin on October 10—"I can't find anything to talk to the working men about this year but your book. I mean to give them a commentary *à la* Coke upon Lyttleton."

The lectures to working men here referred to, six in number, were duly delivered once a week from November 10 onwards, and published in the form of as many little pamphlets. Appearing under the general title, "On our Knowledge of the Causes of the Phenomena of Organic Nature," they wound up with a critical examination of the portion of Mr. Darwin's work *On the Origin of Species*, in relation to the complete theory of the causes of organic nature.

JERMYN STREET, Dec. 2, 1862.

MY DEAR DARWIN—I send you by this post three of my working men's lectures now in course of delivery. As you will see by the prefatory notice, I was asked to allow them to be taken down in shorthand for the use of the audience, but I have no interest in them, and do not desire or intend that they should be widely circulated.

Some time hence, may be, I may revise and illustrate them, and make them into a book as a sort of popular exposition of your views, or at any rate of my version of your views.

There really is nothing new in them nor anything worth your attention, but if in glancing over them at any time you should see anything to object to, I should like to know.

I am very hard worked just now—six lectures a week, and no end of other things—but as vigorous as a three-year old.

I do grudge Hardwicke very much having not only the publisher's but the author's profits. It so often happens that popular lectures designed for a class and inspired by an attentive audience's sympathy are better than any writing in the closet for the purpose of educating the many as readers, and of remunerating the publisher and author. I would lose no time in considering well what steps to take to rescue the copyright of the third thousand.

As for the value of the work thus done in support of Darwin's theory, it is worth while quoting the words of Lord Kelvin, when, as President of the Royal Society in 1894, it fell to him to award Huxley the Darwin Medal:—

To the world at large, perhaps, Mr. Huxley's share in moulding the thesis of *Natural Selection* is less well known than is his bold unwearied exposition and defence of it after it had been made public. And, indeed, a speculative trifler, revelling in the problems of the "might have been," would find a congenial theme in the inquiry how soon what we now call "Darwinism" would have met with the acceptance with which it has met, and gained the power which it has gained, had it not been for the brilliant advocacy with which in its early days it was expounded to all classes of men.

That advocacy had one striking mark: while it made or strove to make clear how deep the new view went down, and how far it reached, it never shrank from trying to make equally clear the limit beyond which it could not go.

you thought I was going to ask you next. But I am not, for rejoiced as I should be to have you, I know you have heaps of better work to do, and hate journalism.

But can you tell me of any plastic young botanist who would come in all for glory and no pay, though I think pay may be got if the concern is properly worked. How about Oliver?

And though you can't and won't be an editor yourself, won't you help us and pat us on the back?

The tone of the *Review* will be mildly episcopophagous, and you and Darwin and Lyell will have a fine opportunity if you wish it of slaying your adversaries.—Ever yours faithfully,
T. H. HUXLEY.

Several of his elder friends tried to dissuade him from an undertaking which would inevitably distract him from his proper work. Sir C. Lyell prophesied (see p. 217) that all the work would drift to the most energetic member of the staff, and Huxley writes to Hooker, August 2, 1860:—

Darwin wrote me a very kind expostulation about it, telling me I ought not to waste myself on other than original work. In reply, however, I assured him that I *must* waste myself willy-nilly, and that the *Review* was only a save-all.

The more I think of it the more it seems to me it ought to answer if properly conducted, and it ought to be of great use.

The first number appeared in January 1861. Writing on the 6th, Huxley says:—

It is pleasant to get such expressions of opinion as I have had from Lyell and Darwin about the *Review*. They make me quite hopeful about its prosperity, as I am sure we shall be able to do better than our first number.

It was not long, however, before Lyell's prophecy began to come true. In June Huxley writes:—

It is no use letting other people look after the journal. I find unless I revise every page of it, it goes wrong.

But in July 1863 he definitely ceased to contribute:—

I did not foresee all this crush of work, (he writes) when the *Review* was first started, or I should not have pledged myself to any share in supplying it. (Moreover, with the appointment

societies was already being mooted, and a letter to Sir C. Lyell gives his ideas thus early not only on this point, but on the general question of women's education.

March 17, 1860.

MY DEAR SIR CHARLES—To use the only forcible expression, I “twig” your meaning perfectly, but I venture to think the parable does not apply. For the Geological Society is not, to my mind, a place of education for students, but a place of discussion for adepts; and the more it is applied to the former purpose the less competent it must become to fulfil the latter—its primary and most important object.

[I am far from wishing to place any obstacle in the way of the intellectual advancement and development of women. On the contrary, I don't see how we are to make any permanent advancement while one-half of the race is sunk, as nine-tenths of women are, in mere ignorant parsonese superstitions; and to show you that my ideas are practical I have fully made up my mind, if I can carry out my own plans, to give my daughters the same training in physical science as their brother will get, so long as he is a boy. They, at any rate, shall not be got up as man-traps for the matrimonial market. If other people would do the like the next generation would see women fit to be the companions of men in all their pursuits—though I don't think that men have anything to fear from their competition. But you know as well as I do that other people won't do the like, and five-sixths of women will stop in the doll stage of evolution to be the stronghold of parsondom, the drag on civilisation, the degradation of every important pursuit with which they mix themselves—“intrigues” in politics, and “friponnes” in science.

If my claws and beak are good for anything they shall be kept from hindering the progress of any science I have to do with.—Ever yours faithfully,

T. H. HUXLEY.)

Three letters to Mr. Spencer show that he had been reading and criticising the proofs of the *First Principles*. With regard to the second letter, which gives reasons for rejecting Mr. Spencer's remarks about the power of inflation in birds during flight, it is curious to note Mr. Spencer's reply:—

How oddly the antagonism comes out even when you are not conscious of it! My authority was Owen! I heard him assign

pose they have troubled me. On the contrary they were at times the only things I could attend to. I agree in the spirit of the whole perfectly. On some matters of detail I had doubts which I am not at present clear-headed enough to think out.

The only thing I object to *in toto* is the illustration which I have marked at p. 24. It is physically impossible that a bird's air-cells should be *distended* with air during flight, unless the structure of the parts is in reality different from anything which anatomists at present know. Blowing into the trachea is not to the point. A bird cannot blow into its own trachea, and it has no mechanism for performing a corresponding action.

A bird's chest is essentially a pair of bellows in which the sternum during rest and the back during flight act as movable wall. The air cells may all be represented as soft-walled bags opening freely into the bellows—there being, so far as anatomists yet know, no valves or corresponding contrivances anywhere except at the glottis, which corresponds with the nozzle and air valve both, of our bellows. But the glottis is always opened when the chest is dilated at each inspiration. How then can the air in any air-cell be kept at a higher tension than the surrounding atmosphere?

Hunter experimented on the uses of the air sacs, I know, but I have not his works at hand. It may be that opening one of the air-cells interferes with flight, but I hold it very difficult to conceive that the interference can take place in the way you suppose. How on earth is a lark to sing for ten minutes together if the air-cells are to be kept distended all the while he is up in the air?

At any rate twenty other illustrations will answer your purpose as well, so I would not select one which may be assailed by a carping fellow like—Yours very faithfully,

T. H. HUXLEY.

Oct. 10, 1860.

MY DEAR SPENCER *—"A wilful man must have his way," and if you won't let me contribute towards the material guarantees for the success of your book, I must be content to add twelve shillings' worth of moral influence to that I already meant to exert per annum in its favour.

I shall be most glad henceforth, as ever, to help your great

* Mr. Spencer had insisted that he should be on the free list, instead of being a paying subscriber to the serial issue of the "Synthetic Philosophy."

together for over forty years without the shadow of a misunderstanding, presupposes an unusually strong friendship firmly based upon mutual trust and respect as well as liking, the beginning of which Sir J. Hooker thus describes :—

My first meeting your father was in 1851, shortly after his return from the *Rattlesnake* voyage with Captain Stanley. Hearing that I had paid some attention to marine zoology during the voyage of the Antarctic Expedition, he was desirous of showing me the results of his studies of the Oceanic Hydrozoa, and he sought me out in consequence. This and the fact that we had both embarked in the Naval service in the same capacity as medical officers and with the same object of scientific research, naturally led to an intimacy which was undisturbed by a shadow of a misunderstanding for nearly forty-five following years. Curiously enough, our intercourse might have dated from an earlier period by nearly six years had I accepted an appointment to the *Rattlesnake* offered me by Captain Stanley, which, but for my having arranged for a journey to India, might have been accepted.

Returning to the purpose of our interview, the researches Mr. Huxley laid before me were chiefly those on the Salpæ, a much misunderstood group of marine Hydrozoa. Of these I had amused myself with making drawings during the long and often weary months passed at sea on board the *Ercbus*, but having other subjects to attend to, I had made no further study of them than as consumers of the vegetable life (Diatoms) of the Antarctic Ocean. Hence his observations on their life-history, habits, and affinities were on almost all points a revelation to me, and I could not fail to recognise in their author all the qualities possessed by a naturalist of commanding ability, industry, and power of exposition. Our interviews, thus commenced, soon ripened into a friendship, which led to an arrangement for a monthly meeting, and in the informal establishment of a club of nine, the other members of which were, Mr. Busk, Dr. Frankland, Mr. Hirst, Sir J. Lubbock, Mr. Herbert Spencer, Dr. Tyndall, and Mr. Spottiswoode.

Just a month after this letter to his friend, the same year which had first brought Huxley public recognition outside his special sphere brought him also the greatest sorrow perhaps of his whole life. I have already spoken

Your letter leads me to think he was right, though not perhaps in the sense he attached to his own words.

To begin with the great doctrine you discuss. I neither deny nor affirm the immortality of man. I see no reason for believing in it, but, on the other hand, I have no means of disproving it.

Pray understand that I have no *a priori* objections to the doctrine. No man who has to deal daily and hourly with nature can trouble himself about *a priori* difficulties. Give me such evidence as would justify me in believing anything else, and I will believe that. Why should I not? It is not half so wonderful as the conservation of force, or the indestructibility of matter. Whoso clearly appreciates all that is implied in the falling of a stone can have no difficulty about any doctrine simply on account of its marvellousness. But the longer I live, the more obvious it is to me that the most sacred act of a man's life is to say and to feel, "I believe such and such to be true." All the greatest rewards and all the heaviest penalties of existence cling about that act. The universe is one and the same throughout; and if the condition of my success in unravelling some little difficulty of anatomy or physiology is that I shall rigorously refuse to put faith in that which does not rest on sufficient evidence, I cannot believe that the great mysteries of existence will be laid open to me on other terms. It is no use to talk to me of analogies and probabilities. I know what I mean when I say I believe in the law of the inverse squares, and I will not rest my life and my hopes upon weaker convictions. I dare not if I would.

Measured by this standard, what becomes of the doctrine of immortality?

You rest in your strong conviction of your personal existence, and in the instinct of the persistence of that existence which is so strong in you as in most men.

To me this is as nothing. That my personality is the surest thing I know—may be true. But the attempt to conceive what it is leads me into mere verbal subtleties. I have champed up all that chaff about the ego and the non-ego, about noumena and phenomena, and all the rest of it, too often not to know that in attempting even to think of these questions, the human intellect flounders at once out of its depth.

It must be twenty years since, a boy, I read Hamilton's essay on the unconditioned, and from that time to this, ontological speculation has been a folly to me. When Mansel took up Hamilton's argument on the side of orthodoxy (!) I said he re-

such a system is indispensable to practical morality. I believe that both these dogmas are very mischievous lies.

With respect to the first, I am no optimist, but I have the firmest belief that the Divine Government (if we may use such a phrase to express the sum of the "customs of matter") is wholly just. The more I know intimately of the lives of other men (to say nothing of my own), the more obvious it is to me that the wicked does *not* flourish nor is the righteous punished. But for this to be clear we must bear in mind what almost all forget, that the rewards of life are contingent upon obedience to the *whole* law—physical as well as moral—and that moral obedience will not atone for physical sin, or *vice versa*.

The ledger of the Almighty is strictly kept, and every one of us has the balance of his operations paid over to him at the end of every minute of his existence.

Life cannot exist without a certain conformity to the surrounding universe—that conformity involves a certain amount of happiness in excess of pain. In short, as we live we are paid for living.

And it is to be recollected in view of the apparent discrepancy between men's acts and their rewards that Nature is juster than we. She takes into account what a man brings with him into the world, which human justice cannot do. If I, born a bloodthirsty and savage brute, inheriting these qualities from others, kill you, my fellow-men will very justly hang me, but I shall not be visited with the horrible remorse which would be my real punishment if, my nature being higher, I had done the same thing.

The absolute justice of the system of things is as clear to me as any scientific fact. The gravitation of sin to sorrow is as certain as that of the earth to the sun, and more so—for experimental proof of the fact is within reach of us all—nay, is before us all in our own lives, if we had but the eyes to see it.

Not only, then, do I disbelieve in the need for compensation, but I believe that the seeking for rewards and punishments out of this life leads men to a ruinous ignorance of the fact that their inevitable rewards and punishments are here.

If the expectation of hell hereafter can keep me from evil-doing, surely *a fortiori* the certainty of hell now will do so? If a man could be firmly impressed with the belief that stealing damaged him as much as swallowing arsenic would do (and it does), would not the dissuasive force of that belief be greater than that of any based on mere future expectations?

position is. I may be quite wrong, and in that case I know I shall have to pay the penalty for being wrong. But I can only say with Luther, "Gott helfe mir, Ich kann nichts anders."

I know right well that 99 out of 100 of my fellows would call me atheist, infidel, and all the other usual hard names. As our laws stand, if the lowest thief steals my coat, my evidence (my opinions being known) would not be received against him.*

But I cannot help it. One thing people shall not call me with justice and that is—a liar. As you say of yourself, I too feel that I lack courage; but if ever the occasion arises when I am bound to speak, I will not shame my boy.

I have spoken more openly and distinctly to you than I ever have to any human being except my wife.

If you can show me that I err in premises or conclusion, I am ready to give up these as I would any other theories. But at any rate you will do me the justice to believe that I have not reached my conclusions without the care befitting the momentous nature of the problems involved.

And I write this the more readily to you, because it is clear to me that if that great and powerful instrument for good or evil, the Church of England, is to be saved from being shivered into fragments by the advancing tide of science—an event I should be very sorry to witness, but which will infallibly occur if men like Samuel of Oxford are to have the guidance of her destinies—it must be by the efforts of men who, like yourself, see your way to the combination of the practice of the Church with the spirit of science. Understand that all the younger men of science whom I know intimately are *essentially* of my way of thinking. (I know not a scoffer or an irreligious or an immoral man among them, but they all regard orthodoxy as you do Brahmanism.) Understand that this new school of the prophets is the only one that can work miracles, the only one that can constantly appeal to nature for evidence that it is right, and you will comprehend that it is of no use to try to barricade us with shovel hats and aprons, or to talk about our doctrines being "shocking."

I don't profess to understand the logic of yourself, Maurice, and the rest of your school, but I have always said I would swear by your truthfulness and sincerity, and that good must come of your efforts. The more plain this was to me, however, the more obvious the necessity to let you see where the men of

* The law with respect to oaths was reformed in 1869.

and let the devil have his own way. And I will be torn to pieces before I am forty sooner than see that.

I have been privately trading on my misfortunes in order to get a little peace and quietness for a few months. If I can help it I don't mean to do any dining out this winter, and I have cut down Societies to the minimum of the Geological, from which I cannot get away.

But it won't do to keep this up too long. By and by one must drift into the stream again, and then there is nothing for it but to pull like mad unless we want to be run down by every collier.

I am going to do one sensible thing, however, viz. to rush down to Llanberis with Busk between Christmas Day and New Year's Day and get my lungs full of hill-air for the coming session.

I was at Down on Saturday and saw Darwin. He seems fairly well, and his daughter was up and looks better than I expected to see her.—Ever yours faithfully,

T. H. HUXLEY.

Meanwhile, he took the opportunity to make the child's birth a new link with his old friend, and wrote as follows:—

14 WAVERLEY PLACE, *Jan.* 3, 1861.

MY DEAR HOOKER—If I had nothing else to write about I must wish you a Happy New Year and many on 'em; but, in fact, my wife and I have a great favour to ask of you, which is neither more nor less than to stand godfather for our little son. You know my opinions on these matters, and I would not ask you to do anything I would not do myself, so if you consent, the clerk shall tell all the lies for you, and you shall be asked to do nothing else than to help devour the christening feed, and be as good a friend to the boy as you have been to his father.

My wife will have the youngster christened, although I am always in a bad temper from the time it is talked about until the ceremony is over. The only way of turning the farce into a reality is by making it an extra bond with one's friends. On the other hand, if you have any objection to say, "all this I steadfastly believe," even by deputy, I know you will have no hesitation in saying so, and in giving me as frank a refusal as my request.*

* As against his dislike of consenting to a rite, to him meaningless, he was moved by a feeling which in part corresponded to Descartes' *morale par provision*,—in part was an acknowledgment of the possi-

into a good many points for myself. The results, when they do come out, will, I foresee, astonish the natives.

I am cold-proof, and all the better for the Welsh trip. To say truth, I was just on the edge of breaking down when I went. Did I ever send you a letter of mine on the teaching of Natural History? It was published while you were away, and I forget whether I sent it or not. However, a copy accompanies this note. . . .

Of course there will be room for your review and welcome. I have put it down and reckon on it.—Ever yours faithfully,

T. H. HUXLEY.

Huxley returned from the trip to Wales in time to be with his wife for the New Year. The plot she had made with Dr. Tyndall had been entirely successful. The threatened breakdown was averted. Wales in winter was as good as Switzerland. Of the ascent of Snowdon he writes on December 28: "Both Tyndall and I voted it under present circumstances as good as most things Alpine."

His wife, however, continued in very weak health. She was prostrated by the loss of her little boy. So in the middle of March he gladly accepted Mr. Darwin's invitation for her and the three children to spend a fortnight in the quiet of his house at Down, where he himself managed to run down for a week end. "It appears to me," he writes to his wife, "that you are subjecting poor Darwin to a savage Tennysonian persecution. I shall see him looking like a martyr and have to talk double science next Sunday."

In April another good friend, Dr. Bence Jones, lent the invalid his house at Folkestone for three months. Unable even to walk when she went there, her recovery was a slow business. Huxley ran down every week; his brother George and his wife also were frequent visitors. Meanwhile he resolved to move into a new house, in order that she might not return to a place so full of sorrowful memories. On May 30 he effected the move to a larger house not half a mile away from Waverley Place—26 Abbey Place (now 23 Abercorn Place). Here also Mrs. Heathorn lived for the next year, my grandfather, over seventy as he was, being

You know all these things as well as I do, and I know as well as you do that such thoughts do not cure heartache or assuage grief. Such maladies, when men are as old as you and I are, are apt to hang about one a long time, but I find that if they are faced and accepted as part of our fair share of life, a great deal of good is to be got out of them. You will find that too, but in the meanwhile don't go and break yourself down with over wear and tear. The heaviest pull comes after the excitement of a catastrophe of this kind is over.

Believe in my affectionate sympathy with you, and that I am, my dear old fellow, yours ever,

T. H. HUXLEY.

And again on the 18th:—

Many thanks for your two letters. It would be sad to hear of life dragging itself out so painfully and slowly, if it were not for what you tell me of the calmness and wisdom with which the poor sufferer uses such strength as is left him.

One can express neither wish nor hope in such a case. With such a man what is will be well. All I have to repeat is, don't knock yourself up. I wish to God I could help you in some way or other beyond repeating the parrot cry. If I can, of course you will let me know.

In June 1861 a jotting in his notebook records that he is at work on the chick's skull, part of the embryological work which he took up vigorously at this time, and at once the continuation of his researches on the Vertebrate Skull, embodied in his Croonian lecture of 1858, and the beginning of a long series of investigations into the structure of birds. There is a reference to this in a very interesting letter dealing chiefly with what he conceived to be the cardinal point of the Darwinian theory:—

26 ABBEY PLACE, *Sept.* 4, 1861.

MY DEAR HOOKER—Yesterday being the first day I went to the Athenæum after reading your note, I had a look at, and a good laugh over, the *Quarterly* article. Who can be the writer?

I have been so busy studying chicken development, a difficult subject to which I had long ago made up my mind to devote my first spare time, that I have written you no word about your article in the *Gardener's Chronicle*. I quite agree with the general tendency of your argument, though it seems to me that

being made responsible with Sabine, and indeed I think he had little enough to do with it.

You will see a letter from him in this week's *Athenæum*.—
Ever yours faithfully, T. H. HUXLEY.

affair was that it absorbed the State aid which might have been given to more valuable researches.

The Council of the Royal Society had been consulted as to the advisability of despatching this expedition and opposed it, for there were in the service of the Company not a few men admirably qualified for the duty, whose scientific services had received scant appreciation. Nevertheless, the expedition started after all, with the approval of Colonel Sabine, the president. In the last months of 1866, Huxley drew up for the Royal Society a report upon the scientific value of the results of the expedition.

CHAPTER XVII

1861-1863

It has been seen that the addition of journalistic work in science to the mass of original research and teaching work upon which Huxley was engaged, called forth a remonstrance from both Lyell and Darwin. To Hooker it seemed still more serious that he was dividing his allegiance, and going far afield in philosophy, instead of concentrating himself upon natural science. He writes:—

I am sorry to hear that you are so poorly, and wish I could help you to sit down and work quietly at pure science. You have got into a whirlpool, and should strike out vigorously at the proper angle, not attempt to breast the whole force of the current, nor yet give in to it. Do take the counsel of a quiet looker on and withdraw to your books and studies in pure Natural History; let modes of thought alone. You may make a very good naturalist, or a very good metaphysician (of that I know nothing, don't despise me), but you have neither time nor place for both.

However, it must be remarked that this love of philosophy, not recently acquired either, was only part of the passion for general principles underlying the facts of science which had always possessed him. And the time expended upon it was not directly taken from the hours of scientific work; he would read in bed through the small hours of the night, when sleep was slow in coming to him. In this way he got through an immense amount of philosophy in the course of several years. Not that he could "state the views of so and so" upon any given question, or desired such kind of knowledge; he wished to find out and compare with his

own the answers which other thinkers gave to the problems which interested himself.

A gentler reproof of this time touches his handwriting, which was never of the most legible, so that his foreign correspondents in particular sometimes complained. Haeckel used to get his difficulties deciphered by his colleague Gegenbaur. I cannot forbear quoting the delicate remonstrance of Professor Lacaze Duthiers, and the flattering remedy he proposed:—

March 14. — Je lis l'Anglais imprimé, mais vos écritures anglaises sont si rapides, qu'il m'est quelquefois difficile de m'en sortir. On me dit que vous écrivez si bien le français que je crois que je vous lirais bien mieux dans ma langue!

On his return from examining at Dublin, he again looked over proofs for Mr. Spencer.

JERMYN STREET, *Aug. 3, 1861.*

MY DEAR SPENCER — I have been absent on a journey to Dublin and elsewhere* nearly all this week, and hence your note and proof did not reach me till yesterday. I have but just had time to glance through the latter, and I need hardly say how heartily I concur in its general tenor. I have, however, marked one or two passages which I think require some qualification. Then, at p. 272, the fact that the vital manifestations of plants depend as entirely as those of animals upon the fall towards stable equilibrium of the elements of a complex protein compound is not sufficiently prominent. It is not so much that plants are deoxidisers and animals oxidisers, as that plants are manufacturers and animals consumers. It is true that plants manufacture a good deal of non-nitrogenous produce in proportion to the nitrogenous, but it is the latter which is chiefly useful to the animal consumer and not the former. This point is a very important one, which I have never seen clearly and distinctly put—the prettiness of Dumas' circulation of the elements having seduced everybody.

Of course this in no way affects the principle of what you say. The statements which I have marked at p. 276 and 278 should have their authorities given, I think. I should hardly like to commit myself to them absolutely.

You will, if my memory does not mislead me, find authority for my note at p. 283 in Stephenson's life. I think old Geo.

* Visiting Sir Philip Egerton at Oulton Park.

October shows an unusual entry in his diary; the sacrifice of a working evening to hear Jenny Lind sing. Fond though he was of music, as those may remember who ever watched his face at the Sunday evening gatherings in Marlborough Place in the later seventies, when there was sure to be at least a little good music or singing either from his daughters or some of the guests, he seldom could spare the time for concert-going or theatre-going, and the occasional notes of his bachelor days, "to the opera with Spencer," had ceased as his necessary occupations grew more engrossing.

This year his friend Hooker moved to Kew to act as second in command to his father, Sir William Hooker, the director of the Botanical Gardens. This move made meetings between the two friends, except at clubs and societies, more difficult, and was one of the immediate causes of the foundation of the *x* Club. It is this move which is referred to in the following letters; the "poor client" being the wife of an old messmate of his on the *Rattlesnake*:—

JERMYN STREET, Nov. 17.

MY DEAR HOOKER—My wife wrote to yours yesterday, the enclosed note explaining the kitchen-revolution which, it seems, must delay our meeting. When she had done, however, she did not know where to direct it, and I am no wiser, so I send it to you.

It's a horrid nuisance and I have sworn a few, but that will not cook the dinner, however much it may prepare me for being cooked elsewhere. To complete my disgust at things in general, my wife is regularly knocked up with dining out twice this week, though it was only in the quietest way. I shall have to lock her up altogether.

X—— has made a horrid mess of it, and I am sorry to say, from what I know of him, that I cannot doubt where the fault lies. The worst of it is that he has a wife and three children over here, left without a penny or any means of support. The poor woman wrote to me the other day, and when I went to see her I found her at the last shilling and contemplating the work-house as her next step. She has brothers in Australia, and it appeared to me that the only way to do her any good was to get her out. She cannot starve there, and there will be more hope

1859, was now published in the *Proceedings of the Linnæan Society*.

In the list of work in hand are four paleontological papers,* besides the slowly progressing *Manual of Comparative Anatomy*.

When he went north to deliver his lectures at Edinburgh "On the relation of Man to the Lower Animals," he took the opportunity of examining fossils at Forfar, and lectured also at Glasgow; while at Easter he went to Ireland; on March 15 he was at Dublin, lecturing there on the 25th.

Reference has already been made (in the letter to C. Darwin of May 6, 1862) to the unsatisfactory state of Huxley's health. He was further crippled by neuralgic rheumatism in his arm and shoulder, and to get rid of this, went on July 1 to Switzerland for a month's holiday. Reaching Grindelwald on the 4th, he was joined on the 6th by Dr. Tyndall, and with him rambled on the glacier and made an expedition to the Faulhorn. On the 13th they went to the Rhone glacier, meeting Sir J. Lubbock on their way, at the other side of the Grimsel. Both here and at the Eggischhorn, where they went a few days later, Huxley confined himself to easy expeditions, or, as his notebook has it, stayed "quiet" or "idle," while the hale pair ascended the Galenstock and the Jungfrau.

By July 28 he was home again in time for an examiners' meeting at the London University the next day, and a *viva voce* in physiology on the 4th August, before going to Scotland to serve on the Fishery Commission.

This was the first of the numerous commissions on which he served. With his colleagues, Dr. Lyon Playfair (afterwards Lord Playfair) and Colonel Maxwell, he was busy from August 8 to September 16, chiefly on the west coast, taking evidence from the trawlers and their oppo-

* "On Indian Fossils," on "Cephalaspis and Pteraspis," on "Stagonolepis," and a "Memoir descriptive of Labyrinthodont remains from the Trias and Coal of Britain," which he first treated of in 1858, "clearly establishing for the first time the vertebrate nature of these remains."—Sir M. Foster, Obit. Notice, *Proc. R. S.* lix. 55.

This autumn he gladly took on what appeared to be an additional piece of work. On October 12 he writes from 26 Abbey Place:—

I saw Flower yesterday, and I find that my present colleague in the Hunterian Professorship wishes to get rid of his share in the lectures, having, I suppose, at the eleventh hour discovered his incompetency. It looks paradoxical to say so, but it will really be easier for me to give eighteen or twenty-four lectures than twelve, so that I have professed my readiness to take as much as he likes off his hands.

This professorship had been in existence for more than sixty years, for when the Museum of the famous anatomist John Hunter was entrusted to the College of Surgeons by the Government, the condition was made that "one course of lectures, not less than twenty-four in number, on comparative anatomy and other subjects, illustrated by the preparations, shall be given every year by some member of the company." Huxley arranged to publish from year to year the substance of his lectures on the vertebrates, "and by that process to bring out eventually a comprehensive, though condensed, systematic work on *Comparative Anatomy*." *

Of the labour entailed in this course, the late Sir W. H. Flower wrote:—

When, in 1862, he was appointed to the Hunterian Professorship at the College of Surgeons, he took for the subject of several yearly courses of lectures the anatomy of the vertebrata, beginning with the primates, and as the subject was then rather new to him, and as it was a rule with him never to make a statement in a lecture which was not founded upon his own actual observation, he set to work to make a series of original dissections of all the forms he treated of. These were carried on in the workroom at the top of the college, and mostly in the evenings, after his daily occupation at Jermyn Street (the School of Mines, as it was then called) was over, an arrangement which my residence in the college buildings enabled me to make for him. These rooms contained a large store of material, entire or partially dissected animals preserved in spirit,

* *Comparative Anatomy*, vol. i. Preface.

The next letter refers to the scientific examinations at the University of London.

Dec. 4, 1862.

MY DEAR HOOKER—I look upon you as art and part of the *Natural History Review*, though not ostensibly one of the gang, so I bid you to a feast, partly of reason and partly of mutton, at my house on December 11 (being this day week) at half-past six. Do come if you can, for we have not seen your ugly old phiz for ages, and should be comforted by an inspection thereof, however brief.

I did my best yesterday to get separate exhibitions for Chemistry, Botany, and Zoological Biology, at the committee yesterday,* and I suspect from your letter that if you had been there you would have backed me. However, it is clear they only mean to give separate exhibits for Chemistry and Biology as a whole.

Because Botany and Zoology are, philosophically speaking, cognate subjects, people are under the delusion that it is easier to work both up at the same time, than it would be to work up, say, Chemistry and Botany. Just fancy asking a young man who has heaps of other things to work up for the B.Sc., to qualify himself for honours both in botany, histological, systematic, and physiological. That is to say, to get a *practical knowledge* of both these groups of subjects.

I really think the botanical and zoological examiners ought to memorialise the senate jointly on the subject. The present system leads to mere sham and cram.—Ever yours,

T. H. HUXLEY.

The year 1863, notable for the publication of Huxley's first book, found him plunged deep in an immense quantity of work of all sorts. He was still examiner in Physiology and Comparative Anatomy at the London University, a post he held from 1855 to 1863, and again from 1865 to 1870, "making," as Sir Michael Foster says, "even an examination feel the influence of the new spirit in biology; and among his examinees at that time there was one at least who, knowing Huxley by his writings, but by his writings only, looked forward to the *viva voce* test, not as a trial, but as an occasion of delight."

* At the London University.

expect an acknowledgment of a book—it is one of the greatest nuisances in the world to have that to do, and I never do it—but as you mentioned the Lectures and not the other, I thought it might not have reached you. If it has not, pray let me know and a copy shall be forwarded, as I want you very much to read Essay No. 2.

I have a great respect for all the old bottles, and if the new wine can be got to go into them and not burst them I shall be very glad—I confess I do not see my way to it; on the contrary, the longer I live and the more I learn the more hopeless to my mind becomes the contradiction between the theory of the universe as understood and expounded by Jewish and Christian theologians, and the theory of the universe which is every day and every year growing out of the application of scientific methods to its phenomena.

Whether astronomy and geology can or cannot be made to agree with the statements as to the matters of fact laid down in Genesis—whether the Gospels are historically true or not—are matters of comparatively small moment in the face of the impassable gulf between the anthropomorphism (however refined) of theology and the passionless impersonality of the unknown and unknowable which science shows everywhere underlying the thin veil of phenomena.

Here seems to me to be the great gulf fixed between science and theology—beside which all Colenso controversies, reconcilements of Scripture *à la* Pye Smith, etc., cut a very small figure.

You must have thought over all this long ago; but steeped as I am in scientific thought from morning till night, the contrast has perhaps a greater vividness to me. I go into society, and except among two or three of my scientific colleagues I find myself alone on these subjects, and as hopelessly at variance with the majority of my fellow-men as they would be with their neighbours if they were set down among the Ashantees. I don't like this state of things for myself—least of all do I see how it will work out for my children. But as my mind is constituted, there is no way out of it, and I can only envy you if you can see things differently.—Ever yours very faithfully,

T. H. HUXLEY.

JERMYN STREET, *May* 5, 1863.

MY DEAR KINGSLEY — My wife and children are away at Felixstow on the Suffolk coast, and as I run down on Saturday and come back on Monday your MS. has been kept longer than

nature and disposition the greatest possible antipathy to all the atheistic and infidel school.

Nevertheless, I know that I am, in spite of myself, exactly what the Christian world call, and, so far as I can see, are justified in calling, atheist and infidel. I cannot see one shadow or tittle of evidence that the great unknown underlying the phenomena of the universe stands to us in the relation of a Father—loves us and cares for us as Christianity asserts. On the contrary, the whole teaching of experience seems to me to show that while the governance (if I may use the term) of the universe is rigorously just and substantially kind and beneficent, there is no more relation of affection between governor and governed than between me and the twelve judges. I know the administrators of the law desire to do their best for everybody, and that they would rather not hurt me than otherwise, but I also know that under certain circumstances they will most assuredly hang me; and that in any case it would be absurd to suppose them guided by any particular affection for me.

This seems to me to be the relation which exists between the cause of the phenomena of this universe and myself. I submit to it with implicit obedience and perfect cheerfulness, and the more because my small intelligence does not see how any other arrangement could possibly be got to work as the world is constituted.

But this is what the Christian world calls atheism, and because all my toil and pains does not enable me to see my way to any other conclusion than this, a Christian judge would (if he knew it) refuse to take my evidence in a court of justice against that of a Christian ticket-of-leave man.

So with regard to the other great Christian dogmas, the immortality of the soul, and the future state of rewards and punishments, what possible objection *a priori* can I—who am compelled perforce to believe in the immortality of what we call Matter and Force and in a very unmistakable *present* state of rewards and punishments for all our deeds—have to these doctrines? Give me a scintilla of evidence, and I am ready to jump at them.

But read Butler, and see to what drivell even his great mind descends when he has to talk about the immortality of the soul! I have never seen an argument on that subject which from a scientific point of view is worth the paper it is written upon. All resolve themselves into this formula:—The doctrine of the

phenomena at present addressing you knows nothing. In fact, if I am pushed, metaphysical speculation lands me exactly where your friend Raphael was when his bitch pupped. In other words, I believe in Hamilton, Mansell and Herbert Spencer so long as they are destructive, and I laugh at their beards as soon as they try to spin their own cobwebs.

Is this basis of ignorance broad enough for you? If you, theologian, can find as firm footing as I, man of science, do on this foundation of minus nought—there will be nought to fear for our ever diverging.

For you see I am quite as ready to admit your doctrine that souls secrete bodies as I am the opposite one that bodies secrete souls—simply because I deny the possibility of obtaining any evidence as to the truth and falsehood of either hypothesis. My fundamental axiom of speculative philosophy is that *materialism and spiritualism are opposite poles of the same absurdity*—the absurdity of imagining that we know anything about either spirit or matter.

Cabanis and Berkeley (I speak of them simply as types of schools) are both asses, the only difference being that one is a black donkey and the other a white one.

This universe is, I conceive, like to a great game being played out, and we poor mortals are allowed to take a hand. By great good fortune the wiser among us have made out some few of the rules of the game, as at present played. We call them "Laws of Nature," and honour them because we find that if we obey them we win something for our pains. The cards are our theories and hypotheses, the tricks our experimental verifications. But what sane man would endeavour to solve this problem: given the rules of a game and the winnings, to find whether the cards are made of pasteboard or gold-leaf? Yet the problem of the metaphysicians is to my mind no saner.

If you tell me that an Ape differs from a Man because the latter has a soul and the ape has not, I can only say it may be so; but I should uncommonly like to know how [you know] either that the ape has not one or that the man has.

And until you satisfy me as to the soundness of your method of investigation, I must adhere to what seems to my mind a simpler form of notation—*i.e.* to suppose that all phenomena have the same substratum (if they have any), and that soul and body, or mental and physical phenomena, are merely diverse manifestations of that hypothetical substratum. In this way,

read it, but I find myself utterly at a loss to comprehend his point of view.—Ever yours faithfully, T. H. HUXLEY.

The following letter is interesting, as showing his continued interest in the question of skull structure, as well as his relation to his friend and fellow-worker, Dr. W. K. Parker.

JERMYN STREET, *March* 18, 1863.

MY DEAR PARKER—Any conclusion that I have reached will seem to me all the better based for knowing that you have been near or at it, and I am therefore right glad to have your letter. If I had only time, nothing would delight me more than to go over your preparations, but these Hunterian Lectures are about the hardest bit of work I ever took in hand, and I am obliged to give every minute to them.

By and by I will gladly go with you over your vast material.

Did you not some time ago tell me that you considered the Y-shaped bone (so-called presphenoid) in the Pike to be the true basisphenoid? If so, let me know before lecture to-morrow, that I may not commit theft unawares.

I have arrived at that conclusion myself from the anatomical relations of the bone in question to the brain and nerves.

I look upon the proposition opisthotis = turtle's "occipital externe" = Perch's Rocher (Cuvier) as the one thing needful to clear up the unity of structure of the bony cranium; and it shall be counted unto me as a great sin if I have helped to keep you back from it. The thing has been dawning upon me ever since I read Kölliker's book two summers ago, but I have never had time to work it out.—Ever yours faithfully,

T. H. HUXLEY.

The following extracts from a letter to Hooker and a letter to Darwin describe the pressure of his work at this time.

1863.

MY DEAR HOOKER— . . . I would willingly send a paper to the Linnæan this year if I could, but I do not see how it is practicable. I lecture five times a week from now till the middle of February. I then have to give eighteen lectures at the Coll. Surgeons—six on classification, and twelve on the vertebrate skeleton. I must write a paper on this new Glyptodon, with some eighteen to twenty plates. A preliminary notice has already gone to the Royal Society. I have a decade of fossil

E. Getting heaps of remains of new Labyrinthodonts from the Glasgow coalfield, which have to be described.

F. Working at a memoir on *Glyptodon* based on a new and almost entire specimen at the College of Surgeons.

G. Preparing a new decade upon Fossil fishes for this place.

H. Knowing that I ought to have written long ago a description of a most interesting lot of Indian fossils sent to me by Oldham.

I. Being blown up by Hooker for doing nothing for the *Natural History Review*.

K. Being bothered by sundry editors just to write articles "which you know you can knock off in a moment."

L. Consciousness of having left unwritten letters which ought to have been written long ago, especially to C. Darwin.

M. General worry and botheration. Ten or twelve people taking up my time all day about their own affairs.

N. O. P. Q. R. S. T. U. V. W. X. Y. Z.

Societies.

Clubs.

Dinners, evening parties, and all the apparatus for wasting time called "Society." Colensoism and botheration about Moses. . . . Finally pestered to death in public and private because I am supposed to be what they call a "Darwinian."

If that is not enough, I could exhaust the Greek alphabet for heads in addition.

I am glad to hear that Wyman thinks well of my book, as he is very competent to judge. I hear it is republished in America, but I suppose I shall get nothing out of it.*

An undated letter to Kingsley, who had suggested that he should write an article on Prayer, belongs probably to the autumn of 1863:—

I should like very much to write such an article as you suggest, but I am very doubtful about undertaking it for *Fraser*. Anything I could say would go to the root of praying altogether, for inasmuch as the whole universe is governed, so far as I can see, in the same way, and the moral world is as much governed by laws as the physical—whatever militates against asking for one sort of blessing seems to me to tell with the same force against asking for any other.

* In this expectation, however, he was agreeably disappointed by the action of D. Appleton and Company, as is told on page 305.

Minister who made the offer that the man of all others for the post would be the late distinguished holder of it, Sir W. H. Flower, a suggestion happily acted on.

Early in August a severe loss befell him in the sudden death of his brother George, who had been his close friend ever since he had returned from Australia, who had given him all the help and sympathy in his struggles that could be given by a man of the world without special interests in science or literature. With brilliancy enough to have won success if he had had patience to ensure it, he was not only a pleasant companion, a "clubbable man" in Johnson's phrase, but a friend to trust. The two households had seen much of one another; the childless couple regarded their brother's children almost as their own. Thus a real gap was made in the family circle, and the trouble was not lessened by the fact that George Huxley's affairs were left in great confusion, and his brother not only spent a great deal of time in looking after the interests of the widow, but took upon himself certain obligations in order to make things straight, with the result that he was even compelled to part with his Royal Medal, the gold of which was worth £50.

CHAPTER XVIII

1864

THE year 1864 was much like 1863. The Hunterian Lectures were still part of his regular work. The Fishery Commission claimed a large portion of his time. From March 28 to April 2 he was in Cornwall; on May 7 at Shoreham; from July 24 to September 9 visiting the coasts of Scotland and Ireland. The same pressure of work continued. He published four papers on paleontological or anatomical subjects in the *Natural History Review*,* he wrote "Further Remarks upon the Human Remains from the Neanderthal," and later (see pp. 273 and 288), dealing with "Criticisms on the *Origin of Species*" (*Collected Essays II.* p. 80, "Darwiniana"), he gently but firmly dispersed several misconceptions of his old friend Kölliker as to the plain meaning of the book; and ridiculed the pretentious ignorance of M. Flourens' dicta upon the same subject; while in the winter he delivered a course of lectures to workingmen on "The Various Races of Mankind," a choice of subject which shows that his chief interest at that time lay in Ethnology.

JERMYN STREET, Jan. 16, 1864.

MY DEAR DARWIN—I have had no news of you for a long time, but I earnestly hope you are better.

Have you any objection* to putting your name to Flower's certificate for the Royal Society herewith inclosed? It will

* On "Cetacean Fossils termed *Ziphius* by Cuvier," in the *Transactions of the Geological Society*; in those of the *Zoological*, papers on "*Arctocebus Calabarensis*" and "the Structure of the Stomach in *Desmodus Rufus*"; and on the "Osteology of the Genus *Glyptodon*," in the *Phil. Trans.*

please him much if you will; and I go bail for his being a thoroughly good man in all senses of the word—which, as you know, is more than I would say for everybody.

Don't write any reply; but Mrs. Darwin perhaps will do me the kindness to send the thing on to Lyell as per enclosed envelope. I will write him a note about it.

We are all well, barring customary colds and various forms of infantile pip. As for myself, I am flourishing like a green bay tree (appropriate comparison, Soapy Sam would observe), in consequence of having utterly renounced societies and society since October.

I have been working like a horse, however, and shall work "horser" as my college lectures begin in February.—*Tout à vous,*
T. H. HUXLEY.

ROYAL SCHOOL OF MINES,
JERMYN STREET, *April 18, 1864.*

MY DEAR DARWIN—I was rejoiced to see your handwriting again, so much so that I shall not scold you for undertaking the needless exertion (as it's my duty to do) of writing to thank me for my book.*

I thought the last lecture would be nuts for you, but it is really shocking. There is not the smallest question that Owen wrote both the article "Oken" and the *Archetype* Book, which appeared in its second edition in French—why, I know not. I think that if you will look at what I say again, there will not be much doubt left in your mind as to the identity of the writer of the two.

The news you give of yourself is most encouraging; but pray don't think of doing any work again yet. Careful as I have been during this last winter not to burn the candle at both ends, I have found myself, since the pressure of my lectures ceased, in considerable need of quiet, and I have been lazy accordingly.

I don't know that I fear, with you, caring too much for science—for there are lots of other things I should like to go into as well, but I do lament more and more as time goes on, the necessity of becoming more and more absorbed in one kind of work, a necessity which is created for any one in my position, partly by one's reputation, and partly by one's children. For directly a man gets the smallest repute in any branch of science, the world immediately credits him with knowing about ten

* *Hunterian Lectures on Anatomy.*

mission's Cruise; and though much fatter (indeed a regular bloater myself), I am not up to the mark. Next year I will have a real holiday.*

I am a bachelor, my wife and belongings being all at that beautiful place, Margate. When I came back I found them all looking so seedy that I took them off bag and baggage to that, as the handiest place, before a week was over. They are wonderfully improved already, my wife especially being abundantly provided with her favourite east wind. Your godson is growing a very sturdy fellow, and I begin to puzzle my head with thinking what he is and what he is not to be taught.

Please to remember me very kindly to Mrs. Darwin, and believe me, yours very faithfully,

T. H. HUXLEY.

The following illustrates the value he set upon public examinations as to a practical means for spreading scientific education, and upon first-rate examiners as a safeguard of proper methods of teaching.

Oct. 6, 1864.

MY DEAR HOOKER—Donnelly told me to-day that you had been applied to by the Science and Arts Department to examine for them in botany, and that you had declined.

Will you reconsider the matter? I have always taken a very great interest in the science examinations, looking upon them, as I do, as the most important engine for forcing science into ordinary education.

The English nation will not take science from above, so it must get it from below.

Having known these examinations from the beginning, I can assure you that they are very genuine things, and are working excellently. And what I have regretted from the first is that the botanical business was not taken in hand by you, instead of by —.

Now, like a good fellow, think better of it. The papers are necessarily very simple, and one of Oliver's pupils could look them over for you. Let us have your co-operation and the advantage of that reputation for honesty and earnestness which you have contrived (Heaven knows how) to get.

I have come back fat and seedy for want of exercise. All

* At the end of the year, as so often, he went off for a ploy with Tyndall, this time into Derbyshire, walking vigorously over the moors.

only part of the address to Darwin written by Sabine himself contained the following passage:—

“Speaking generally and collectively, we have expressly omitted it (Darwin’s theory) from the grounds of our award.”

Of course this would be interpreted by everybody as meaning that, after due discussion, the council had formally resolved not only to exclude Darwin’s theory from the grounds of the award, but to give public notice through the president that they had done so, and furthermore, that Darwin’s friends had been base enough to accept an honour for him on the understanding that in receiving it he should be publicly insulted!

I felt that this would never do, and therefore when the resolution for printing the address was moved, I made a speech which I took care to keep perfectly cool and temperate, disavowing all intention of interfering with the liberty of the president to say what he pleased, but exercising my constitutional right of requiring the minutes of council making the award to be read, in order that the Society might be informed whether the conditions implied by Sabine had been imposed or not.

The resolution was read, and of course nothing of the kind appeared. Sabine didn’t exactly like it, I believe. Both Busk and Falconer remonstrated against the passage to him, and I hope it will be withdrawn when the address is printed.*

If not there will be an awful row, and I for one will show no mercy.—Ever yours faithfully,

T. H. HUXLEY.

The foundation of the π Club towards the end of 1864 was a notable event for Huxley and his circle of scientific friends. It was growing more and more difficult for them to see one another except now and again at meetings of the learned societies, and even that was quite uncertain. The pressure of Huxley’s own work may be inferred from his letters at this time (especially to Darwin, July 2, 1863, and January 16, 1864). Not only society, but societies had to be almost entirely given up. Moreover, the distance from one another at which some of these friends lived, added another difficulty, so that Huxley writes to Hooker in his “remote province” of Kew: “I wonder if we are ever to meet again in this world.” Accordingly in January 1864,

* The passage stands in the published address, but followed by another passage which softens it down.

men with such opportunities of mutual understanding and such ideals of action could not fail to have some influence on the progress of scientific organization—it was assuredly not sectarian nor exerted for party purposes during the twenty-eight years of the club's existence.

I believe that the α (continues Huxley) had the credit of being a sort of scientific caucus, or ring, with some people. In fact, two distinguished scientific colleagues of mine once carried on a conversation (which I gravely ignored) across me, in the smoking-room of the Athenæum, to this effect, "I say, A., do you know anything about the α Club?" "Oh yes, B., I have heard of it. What do they do?" "Well, they govern scientific affairs, and really, on the whole, they don't do it badly." If my good friends could only have been present at a few of our meetings, they would have formed a much less exalted idea of us, and would, I fear, have been much shocked at the sadly frivolous tone of our ordinary conversation.

The α club is probably unique in the smallness of its numbers, the intellectual eminence of its members, and the length of its unchanged existence. The nearest parallel is to be found in "The Club." * Like the α , "The Club" began with eight members at its first meeting, and of the original members Johnson lived twenty years, Reynolds twenty-eight, Burke thirty-three, and Bennet Langton thirty-seven. But the ranks were earlier broken. Within ten years Goldsmith died, and he was followed in a twelvemonth by Nugent, and five years later by Beauclerk and Chamier. Moreover, the eight were soon increased to twelve; then to twenty and finally to forty, while the gaps were filled up as they occurred.

In the α , on the contrary, nearly nineteen years passed before the original circle was broken by the death of Spottiswoode. From 1864 to Spottiswoode's death in 1883 the original circle remained unbroken; the meetings "were steadily continued for some twenty years, before our ranks began to thin; and one by one, *geistige Naturen* such as

* Of which Huxley was elected a member in 1884. Tyndall and Hooker were also members.

However, I am not going to stand out against the general wish, and I shall agree to anything that is desired.

Again—

The club has never had any purpose except the purely personal object of bringing together a few friends who did not want to drift apart. It has happened that these cronies had developed into big-wigs of various kinds, and therefore the club has incidentally—I might say accidentally—had a good deal of influence in the scientific world. But if I had to propose to a man to join, and he were to say, Well, what is your object? I should have to reply like the needy knife-grinder, “Object, God bless you, sir, we’ve none to show.”

As he wrote elsewhere (*Nineteenth Century*, January 1894, see p. 277):—

Later on, there were attempts to add other members, which at last became wearisome, and had to be arrested by the agreement that no proposition of that kind should be entertained, unless the name of the new member suggested contained all the consonants absent from the names of the old ones. In the lack of Slavonic friends this decision put an end to the possibility of increase.

After the death, in February 1892, of Hirst, a most devoted supporter of the club, who “would, I believe, represent it in his sole person rather than pass the day over,” only one more meeting took place, in the following month. With five of the six survivors domiciled far from town, meeting after meeting fell through, until the treasurer wrote, “My idea is that it is best to let it die out unobserved, and say nothing about its decease to anyone.”

Thus it came to pass that the March meeting of the club in 1892 remained its last. No ceremony ushered it out of existence. Its end exemplified a saying of Sir J. Hooker’s, “At our ages clubs are an anachronism.” It had met 240 times, yet, curious to say, although the average attendance up to 1883 was seven out of nine, the full strength of the club only met on twenty-seven occasions.

1866-67; papers on "Two Widely Contrasted Forms of the Human Cranium" of 1866 and 1868; the "Patagonian Skulls" of 1868; and "Some Fixed Points in British Ethnology" of 1870—

His published ethnological papers (says Sir Michael Foster) are not numerous, nor can they be taken as a measure of his influence on this branch of study. In many ways he has made himself felt, not the least by the severity with which on the one hand he repressed the pretensions of shallow persons who, taking advantage of the glamour of the Darwinian doctrine, talked nonsense in the name of anthropological science, and on the other hand, exposed those who in the structure of the brain or of other parts, saw an impassable gulf between man and the monkey. The episode of the "hippocampus" stirred for a while not only science but the general public. He used his influence, already year by year growing more and more powerful, to keep the study of the natural history of man within its proper lines, and chiefly with this end in view held the Presidential Chair of the Ethnological Society in 1869-70. It was mainly through his influence that this older Ethnological Society was, a year later, in 1871, amalgamated with a newer rival society, the Anthropological, under the title of "The Anthropological Institute." *

During this time he was constantly occupied with paleontological work, as the following letter to Sir C. Lyell indicates—

JERMYN STREET, Nov. 27, 1865.

MY DEAR SIR CHARLES—I returned last night from a hasty journey to Ireland, whither I betook myself on Thursday night, being attracted vulture-wise by the scent of a quantity of carboniferous corpses. The journey was as well worth the trouble as any I ever undertook, seeing that in a morning's work I turned out ten genera of vertebrate animals of which five are certainly new; and of these four are *Labyrinthodonts*, *amphibia* of new types. These four are baptised *Ophiderpeton*, *Lepterpeton*, *Ichthyerpeton*, *Keraterpeton*. They all have ossified spinal columns and limbs. The special interest attaching to the two first is that they represent a type of *Labyrinthodonts* hitherto unknown, and corresponding with *Siren* and *Amphiuma* among living *Amphibia*. *Ophiderpeton*, for example, is like an eel, about three feet long with small fore legs and rudimentary hind ones.

* See Appendix.

half done for more than a twelvemonth, and I hate the sight of them because the subject no longer interests me, and my head is full of other matters.

So I have just done giving a set of lectures to working-men on "The Various Races of Mankind," which really would make a book in Miss Emma's sense of the word, and which I have had reported. But when am I to work them up? Twenty-four Hunterian Lectures loom between me and Easter. I am dying to get out the second volume of the book that is not a book, but in vain.

I trust you are better, though the last news I had of you from Lubbock was not so encouraging as I could have wished.

With best wishes and remembrances to Mrs. Darwin—Ever yours,
T. H. HUXLEY.

Thanks for "Für Darwin," * I had it.

26 ABBEY PLACE, Jan. 15, 1865.

MY DEAR DARWIN—Many thanks for Deslongchamps' paper which I do not possess.

I received another important publication yesterday morning in the shape of a small but hearty son, who came to light a little before six. The wife is getting on capitally, and we are both greatly rejoiced at having another boy, as your godson ran great risks of being spoiled by a harem of sisters.

The leader in the *Reader* is mine, and I am glad you like it. The more so as it has got me into trouble with some of my friends. However, the revolution that is going on is not to be made with rose-water.

I wish if anything occurs to you that would improve the scientific part of the *Reader*, you would let me know as I am in great measure responsible for it.

I am sorry not to have a better account of your health. With kind remembrances to Mrs. Darwin and the rest of your circle—Ever yours faithfully,
T. H. HUXLEY.

JERMYN STREET, May 1, 1865.

MY DEAR DARWIN—I send you by this post a booklet † none of which is much worth your reading, while of nine-tenths of it you may say as the man did who had been trying to read Johnson's *Dictionary*, "that the words were fine, but he couldn't make much of the story."

* By Fritz Müller, one of Darwin's earliest supporters in Germany.

† Probably "A Catalogue of the Collection of Fossils in the Museum of Practical Geology," etc.

dren, the first time I have had a holiday of any extent with them for years.

We are all flourishing—the babies particularly so—and I find myself rather loth to begin grinding at the mill again. There is a vein of laziness in me which crops out uncommonly strong in your godson, who is about the idlest, jolliest young four year old I know.

You will have been as much grieved as I have been about dear old Hooker. According to the last accounts, however, he is mending, and I hope to see him in the pristine vigour again before long.

My wife is gone to bed or she would join me in the kindest regards and remembrances to Mrs. Darwin and your family.—
Ever yours faithfully, T. H. HUXLEY.

The sound judgment and nice sense of honour for which Huxley was known among his friends often led those who were in difficulties to appeal to him for advice. About this time a dispute arose over an alleged case of unacknowledged “conveyance” of information. Writing to Hooker, he says the one party to the quarrel failed to “set the affair straight with half a dozen words of frank explanation as he might have done;” as to the other, “like all quiet and mild men who do get a grievance, he became about twice as ‘wud’ as Berserks like you and me.” Both came to him, so that he says, “I have found it very difficult to deal honestly with both sides without betraying the confidence of either or making matters worse.” Happily, with his help, matters reached a peaceful solution, and his final comment is—

I don’t mind fighting to the death in a good big row, but when A and B are supplying themselves from C’s orchard, I don’t think it is very much worth while to dispute whether B filled his pockets directly from the trees or indirectly helped himself to the contents of A’s basket. If B has so helped himself, he certainly ought to say so like a man, but if I were A, I would not much care whether he did or not.

— has been horribly disgusted about it, but I am not sure the discipline may not have opened his eyes to new and useful aspects of nature.

The summer of 1865 saw the inception of an educational experiment—an International Education Society—to which

good-fellowship which existed between them, as well as the interest he took in the style and success of Parker's work. Parker was hard at work on *Birds*, a subject in which his friend and leader also was deeply interested, and was indeed preparing an important book upon it.

Referring to his candidature for the Royal Society, he writes on February 21, 1865: "With reference to your candidature, I am ready to bring your name forward whenever you like, and to back you with 'all my might, power, amity, and authority,' as Essex did Bacon (you need not serve me as Bacon did Essex afterwards), but my impression has been that you did not wish to come forward this year."

And on November 2, 1866, congratulating him on his "well-earned honour" of the F.R.S.—"Go on and prosper. These are not the things wise men work for; but it is not the less proper of a wise man to take them when they come unsought."

26 ABBEY PLACE, *Dec.* 3, 1865.

MY DEAR PARKER—I have been so terribly pressed by my work that I have only just been able to finish the reading of your paper.

Very few pieces of work which have fallen in my way come near your account of the *Struthious* skull in point of clearness and completeness. It is a most admirable essay, and will make an epoch in this kind of inquiry.

I want you, however, to remodel the introduction, and to make some unessential but convenient difference in the arrangement of some of the figures.

Secondly, full as the appendix is of most valuable and interesting matter, I advise you for the present to keep it back.

My reason is that you have done justice neither to yourself nor to your topics, and that if the appendix is printed as it stands, your labour will be in great measure lost.

You start subjects enough for half a dozen papers, and partly from the compression thus resulting, and partly from the

worst of the contest." He speaks too of his "minute accuracy in observation and boundless memory for details and imagination which absolutely rioted in the scenting out of subtle and often far-fetched analogies."

CHAPTER XX

1866

BESIDES his Fullerian lectures on Ethnology at the Royal Institution this year, Huxley published in February 1866 a paper in the *Natural History Review*, on the "Pre-historic Remains of Caithness," based upon a quantity of remains found the previous autumn at Keiss. This, and the article on the "Neanderthal Skull" in the *Natural History Review* for 1864, attracted some notice among foreign anthropologists. Dr. H. Welcker writes about them; Dr. A. Ecker wants the "Prehistoric Remains" for his new *Archiv für Anthropologie*; the Société d'Anthropologie de Paris elects him a Foreign Associate.

He was asked by Dr. Fayrer to assist in a great scheme he had proposed to the Asiatic Society,* to gather men of every tribe from India, the Malayan Peninsula, Persia, Arabia, the Indian Archipelago, etc., for anthropological purposes. It was well received by the Council of the Society and by the Lieutenant-Governor of Bengal; anything Huxley could say in its favour would be of great weight. Would he come out as Dr. Fayrer's guest?

Unable to go to Calcutta, he sent the following letter:—

JERMYN STREET, LONDON, *June* 14, 1866.

MY DEAR FAYRER—I lose no time in replying to your second letter, and my first business is to apologise for not having answered the first, but it reached me in the thick of my lectures, and like a great many other things which ought to have been done I put off replying to a more convenient season. I have

* Comp. Chap. XXII. *ad init.* and Appendix I.

your congress. I have been lecturing on Ethnology this year,* and shall be again this year, and I would give a good deal to be able to look at the complex facts of Indian Ethnology with my own eyes.

But as the sage observed, "what's impossible can't be," and what with short holidays—a wife and seven children—and miles of work in arrear, India is an impossibility for me.

You say nothing about yourself, so I trust you are well and hearty, and all your belongings flourishing.—Ever yours faithfully,
T. H. HUXLEY.

In paleontology he published this year papers on the "Vertebrate Remains from the Jarrow Colliery, Kilkenny;" on a new "Telerpeton from Elgin," and on some "Dinosaurs from South Africa." The latter, and many more afterwards, were sent over by a young man named Alfred Brown, who had a curious history. A Quaker gentleman came across him when employed in cleaning tools in Cirencester College, found that he was a good Greek and Latin scholar, and got him a tutorship in a clergyman's family at the Cape. He afterwards entered the postal service, and being inspired with a vivid interest in geology, spent all the leave he could obtain from his office on the Orange River in getting fossils from the Stormberg Rocks. These, as often as he could afford to send such weighty packages, he sent to Sir R. Murchison, to whom he had received a letter of introduction from his official superior. Sir Roderick, writing to Huxley, says "that he was proud of his new recruit," to whom he sent not only welcome words of encouragement, but the no less welcome news that the brother of his "discoverer," hearing of the facts from Professor Woodward, offered to defray his expenses so that he could collect regularly.

On April 2 Huxley was in Edinburgh to receive the first academic distinction conferred upon him in Britain. He received the honorary degree of the University in company with Tyndall and Carlyle. It was part of the fitness of things that he should be associated in this honour with his close friend Tyndall; but though he frequently acknowl-

* As Fullerian Professor at the Royal Institution.

Some whales have all the cerebral vertebrae free *now*, and every one of them has the full number, seven, whether they are free or fixed. No doubt whales had hind legs once upon a time. If when you come up to town you go to the College of Surgeons, my friend Flower the Conservator (a good man whom you should know), will show you the whalebone whale's thigh bones in the grand skeleton they have recently set up. The legs, to be sure, and the feet are gone, the battle of life having left private Cetacea in the condition of a Chelsea pensioner.—Ever yours faithfully,

T. H. HUXLEY.

This year the British Association met at Nottingham, and Huxley was president of Section D. In this capacity he invited Professor Haeckel to attend the meeting, but the impending war with Austria prevented any Prussian from leaving his country at the time, though Haeckel managed to come over later.

Huxley did not deliver a regular opening address to the section on the Thursday, but on the Friday made a speech, which was followed by a discussion upon biology and its several branches, especially morphology and its relation to physiology ("the facts concerning form are questions of force, every form is force visible.") He lamented that the subdivisions of the section had to meet separately as a result of specialisation, the reason for which he found in the want of proper scientific education in schools. And this was the fault of the universities, for just as in the story, "Stick won't beat dog, dog won't bite pig, and so the old woman can't get home," science would not be taught in the schools until it is recognised by the universities.

This prepared the way for Dean Farrar's paper on science teaching in the public schools. His experience as a master at Harrow made him strongly oppose the existing plan of teaching all boys classical composition whether they were suited for it or no. He wished to exchange a great deal of Latin verse-making for elementary science.

This paper was doubly interesting to Huxley, as coming from a classical master in a public school, and he remarked, "He felt sure that at the present time, the important question for England was not the duration of her coal, but the

deed the last was that you were so rampageous you meant to come to London and have a spree among its dissipations. May that be true.

I am in the thick of my work, and have only had time to glance at your *Historical Sketch*.

What an unmerciful basting you give "our mutual friend." I did not know he had put forward any claim! and even now that I read it in black and white, I can hardly believe it.

I am glad to hear from Spencer that you are on the right (that is *my*) side in the Jamaica business. But it is wonderful how people who commonly act together are divided about it.

My wife joins with me in kindest wishes to Mrs. Darwin and yourself—Ever yours faithfully, T. H. HUXLEY.

You will receive an elementary physiology book, not for your reading but for Miss Darwin's. Were you not charmed with Haeckel?

The "Jamaica business" here alluded to was Governor Eyre's suppression of a negro rising, in the course of which he had executed, under martial law, a coloured leader and member of the Assembly, named Gordon. The question of his justification in so doing stirred England profoundly. It became the touchstone of ultimate political convictions. Men who had little concern for ordinary politics, came forward to defend a great constitutional principle which they conceived to be endangered. A committee was formed to prosecute Governor Eyre on a charge of murder, in order to vindicate the right of a prisoner to trial by due process of law. Thereupon a counter-committee was organised for the defence of the man who, like Cromwell, judged that the people preferred their real security to forms, and had presumably saved the white population of Jamaica by striking promptly at the focus of rebellion.

The *Pall Mall Gazette* of October 29, 1866, made a would-be smart allusion to the part taken in the affair by Huxley, which evoked, in reply, a calm statement of his reasons for joining the prosecuting committee:—

It is amusing (says the *Pall Mall*) to see how the rival committees, the one for the prosecution and the other for the defence of Mr. Eyre, parade the names of distinguished persons who are

he is an evil and troublesome person, an English court of justice will certainly find that virtuous person guilty of murder. Nor will the verdict be affected by any evidence that the defendant acted from the best of motives, and, on the whole, did the State a service.

Now, it *may* be that Mr. Eyre was actuated by the best of motives; it *may* be that Jamaica is all the better for being rid of Mr. Gordon; but nevertheless the Royal Commissioners, who were appointed to inquire into Mr. Gordon's case, among other matters, have declared that:—

The evidence, oral and documentary, appears to us to be wholly insufficient to establish the charge upon which the prisoner took his trial. (*Report*, p. 37.)

And again that they

Cannot see in the evidence which has been adduced, any sufficient proof, either of his (Mr. Gordon's) complicity in the outbreak at Morant Bay, or of his having been a party to any general conspiracy against the Government. (*Report*, p. 38.)

Unless the Royal Commissioners have greatly erred, therefore, the killing of Mr. Gordon can only be defended on the ground that he was a bad and troublesome man; in short, that although he might not be guilty, it served him right.

I entertain so deeply-rooted an objection to this method of killing people—the act itself appears to me to be so frightful a precedent, that I desire to see it stigmatised by the highest authority as a crime. And I have joined the committee which proposes to indict Mr. Eyre, in the hope that I may hear a court of justice declare that the only defence which can be set up (if the Royal Commissioners are right) is no defence, and that the killing of Mr. Gordon was the greatest offence known to the law—murder.—I remain, Sir, your obedient servant,

THOMAS H. HUXLEY.

THE ATHENÆUM CLUB, *Oct.* 30, 1866.

Two letters to friends who had taken the opposite side in this burning question show how resolutely he set himself against permitting a difference on matters of principle to affect personal relations with his warmest opponents.

JERMYN STREET, *Nov.* 8, 1866.

MY DEAR KINGSLEY—The letter of which you have heard, containing my reasons for becoming a member of the Jamaica

it will help a great many people to find out what their profoundest political beliefs are.

The hero-worshippers who believe that the world is to be governed by its great men, who are to lead the little ones, justly if they can; but if not, unjustly drive or kick them the right way, will sympathise with Mr. Eyre.

The other sect (to which I belong) who look upon hero-worship as no better than any other idolatry, and upon the attitude of mind of the hero-worshipper as essentially immoral; who think it is better for a man to go wrong in freedom than to go right in chains; who look upon the observance of inflexible justice as between man and man as of far greater importance than even the preservation of social order, will believe that Mr. Eyre has committed one of the greatest crimes of which a person in authority can be guilty, and will strain every nerve to obtain a declaration that their belief is in accordance with the law of England.*

People who differ on fundamentals are not likely to convert one another. To you, as to my dear friend Tyndall, with whom I almost always act, but who in this matter is as much opposed to me as you are, I can only say, let us be strong enough and wise enough to fight the question out as a matter of principle and without bitterness.—Ever yours faithfully,

T. H. HUXLEY.

November 9, 1866.

MY DEAR TYNDALL—Many thanks for the kind note which accompanied your letter to the Jamaica Committee.

When I presented myself at Rogers' dinner last night I had not heard of the latter, and Gassiot began poking fun at me, and declaring that your absence was due to a quarrel between us on this unhappy subject.

I replied to the jest earnestly enough, that I hoped and believed our old friendship was strong enough to stand any strain that might be put on it, much as I grieved that we should be ranged in opposite camps in this or any other cause.

That you and I have fundamentally different political principles must, I think, have become obvious to both of us during the progress of the American War. The fact is made still more plain by your printed letter, the tone and spirit of which I greatly admired without being able to recognise in it any important fact or argument which had not passed through my mind before I joined the Jamaica Committee.

* See Appendix.

CHAPTER XXI

1867

It has already been noted that Huxley's ethnological work continued this year with a second series of lectures at the Royal Institution, while he enlarged his paper on "Two widely contrasted forms of Human Crania," and published it in the *Journal of Anatomy*. One paleontological memoir of his appeared this year on *Acanthopholis*, a fossil from the chalk marl, an additional piece of work for which he excuses himself to Sir C. Lyell (January 4, 1867):—

The new reptile advertised in *Geol. Mag.* has turned up in the way of business, and I could not help giving a notice of it, or I should not have undertaken anything fresh just now.

The Spitzbergen things are very different, and I have taken sundry looks at them and put them by again to let my thoughts ripen.

They are Ichthyosaurian, and I am not sure they do not belong to two species. But it is an awful business to compare all the Ichthyosaurians. I *think* that one form is new. Please to tell Nordenskiöld this much.

However, his chief interest was in the anatomy of birds, at which he had been working for some time, and especially the development of certain of the cranial bones as a basis of classification. On April 11, expanding one of his Hunterian Lectures, he read a paper on this subject at the Zoological Society, afterwards published in their *Proceedings* for 1867.

As he had found the works of Professor Cornay of help in the preparation of this paper, he was careful to send him a copy with an acknowledgment of his indebtedness, elic-

ing the reply, "*c'est si beau de trouver chez l'homme la science unie à la justice.*"

He followed this up with another paper on "The Classification and Distribution of the Alectoromorphae and Heteromorphae" in 1868, and to the work upon this the following letter to his ally, W. K. Parker, refers:—

ROYAL GEOLOG. SURVEY OF GT. BRITAIN,
JERMYN STREET, *July 17, 1867.*

MY DEAR PARKER—Nothing short of the direct temptation of the evil one could lead you to entertain so monstrous a doctrine, as that you propound about *Cariamidae*.

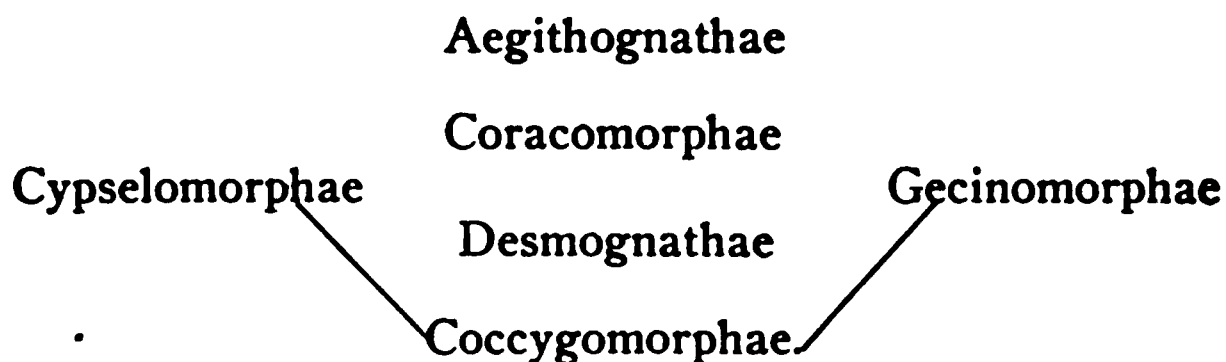
I recommend fasting for three days and the application of a scourge thrice in the twenty-four hours! Do this, and about the fourth day you will perceive that the cranial differences alone are as great as those between *Cathartes* and *Serpentarius*.

If you want to hear something new and true it is this:—

1. That *Memora* is more unlike all the other Passerines (*i.e.* Coracomorphae) than they are unlike one another, and that it will have to stand in a group by itself.

It is as much like a wren as you are—less so, in fact, if you go on maintaining that preposterous fiction about *Serpentarius*.

2. Wood-peckers are more like crows than they are like cuckoos.



3. Sundevall is the sharpest fellow who has written on the classification of birds.

4. Nitzsch and W. K. Parker * are the sharpest fellows who have written on their osteology.

5. Though I do not see how it follows naturally on the above, still, where can I see a good skeleton of *Glareola*?

None in college, B.M.S. badly prepared.—Ever yours faithfully,
T. H. HUXLEY.

* Except in the case of *Serpentarius*.

An incident which diversified one of the Gilchrist lectures to working men is thus recorded by the *Times* of January 23, 1867:—

A GOOD EXAMPLE. Last night, at the termination of a lecture on ethnology, delivered by Professor Huxley to an audience which filled the theatre of the London Mechanics' Institute in Southampton Buildings, Chancery Lane, the lecturer said that he had received a letter as he entered the building which he would not take the responsibility of declining to read, although it had no reference to the subject under consideration. He then read the letter, which was simply signed "A Regular Attendant at Your Lectures," and which in a few words drew attention to the appalling distress existing among the population out of work at the East End, and suggested that all those present at the lecture that night should be allowed the opportunity of contributing 1d. or 2d. each towards a fund for their relief, and that the professor should become the treasurer for the evening. This suggestion was received by the audience with marks of approval. The professor said he would not put pressure on anyone; he would simply place his own subscription in one of the skulls on the table. This he did, and all the audience coming on the platform, threw in money in copper and silver until the novel cash box was filled with coin which amounted to a large sum. A gentleman present expressed a hope that the example set by that audience might be followed with good results wherever large bodies assembled either for educational or recreative purposes.

At the end of April this year my father spent a week in Brittany with Dr. Hooker and Sir J. Lubbock, rambling about the neighbourhood of Rennes and Vannes, and combining the examination of prehistoric remains with the refreshment of holiday making.

Few letters of this period exist. The *x* Club was doing its work. Most of those to whom he would naturally have written he met constantly. Two letters to Professor Haeckel give pieces of his experience. One suggests the limits of aggressive polemics, as to which I remember his once saying that he himself had only twice been the aggressor in controversy, without waiting to be personally attacked; once where he found his opponent was engaged

sympathise entirely with you; and I am much inclined to think that it is a good thing for a man, once at any rate in his life, to perform a public war-dance against all sorts of humbug and imposture.

But having satisfied one's love of freedom in this way, perhaps the sooner the war-paint is off the better. It has no virtue except as a sign of one's own frame of mind and determination, and when that is once known, is little better than a distraction.

I think there are a few patches of this kind, my dear friend, which may as well come out in the next edition, *e.g.* that wonderful note about the relation of God to gas, the gravity of which greatly tickled my fancy.

I pictured to myself the effect which a translation of this would have upon the minds of my respectable countrymen!

Apropos of translation. Darwin wrote to me on that subject, and with his usual generosity, would have made a considerable contribution towards the expense if we could have seen our way to the publication of a translation. But I do not think it would be well to translate the book in fragments, and, as a whole, it would be a very costly undertaking, with very little chance of finding readers.

I do not believe that in the British Islands there are fifty people who are competent to read the book, and of the fifty, five and twenty have read it or will read it in German.

What I desire to do is to write a review of it, which will bring it into some notice on this side of the water, and this I hope to do before long. If I do not it will be, you well know, from no want of inclination, but simply from lack of time.

In any case, as soon as I have been able to study the book carefully, you shall have my honest opinion about all points.

I am glad your journey has yielded so good a scientific harvest, and especially that you found my *Oceanic Hydrozoa* of some use. But I am shocked to find you had no copy of the book of your own, and I shall take care that one is sent to you. It is my first-born work, done when I was very raw and inexperienced, and had neither friends nor help. Perhaps I am all the fonder of the child on that ground.

A lively memory of you remains in my house, and wife and children will be very glad to hear that I have news of you when I go home to dinner.

Keep us in kindly recollection, and believe me—Ever yours
very faithfully,

T. H. HUXLEY.

Station at Naples, on his first visit to England, made my father's acquaintance by accepting his invitation to stay with him "for as long as you can make it convenient to stay" at Swanage, "a little country town with no sort of amusement except what is to be got by walking about a rather pretty country. But having warned you of this, I repeat that it will give me much pleasure to see you if you think it worth while to come so far."

Dr. Dohrn came, and came into the midst of the family—seven children, ranging from ten years to babyhood, with whom he made himself as popular by his farmyard repertory, as he did with the elders by other qualities. The impression left upon him appears from a letter written soon after—

"Ich habe heute mehrere Capitel in Mill's *Utilitarianism* gelesen and das Wort happiness mehr als einmal gefunden: hätte *ich* eine Definition dieses vielumworbenen Wortes irgend Jemand zu geben, ich würde sagen: * go and see the Huxley family at Swanage; and if you would enjoy the same I enjoyed, you would feel what is happiness, and never more ask for a definition of this sentiment."

SWANAGE, *Sept.* 22, 1867.

MY DEAR DOHRN—Thanks to my acquaintance with the *Mikroskopische Anatomie*, and to the fact that you employ our manuscript characters, and not the hieroglyphics of what I venture to call the "cursed" and not "cursiv" Schrift, your letter was as easy as it was pleasant to read. We are all glad to have news of you, though it was really very unnecessary to thank us for trying to make your brief visit a pleasant one. Your conscience must be more "pungent" than your talk, if it pricks you with so little cause. My wife rejoices saucily to find that phrase of hers has stuck so strongly in your mind, but you must remember her fondness for "Tusch."

You must certainly marry. In my bachelor days, it was unsafe for anyone to approach me before mid-day, and for all intellectual purposes I was barren till the evening. Breakfast at six would have upset me for the day. You and the lobster noted the difference the other day.

* I have been reading several chapters of Mill's *Utilitarianism* to-day, and met with the word "happiness" more than once; if I had to give anybody a definition of this much debated word, I should say—

Everything seems to have gone well at the meeting, the educational business carried [*i.e.* a recommendation that natural science be made a part of the curriculum in the public schools], and the anthropologers making fools of themselves in a most effectual way. So that I do not feel I have anything to reproach myself with for being absent.

I am very pleased to hear of the reconciliation with Thomson and Tait. The mode of it speaks well for them, and the fact will remove a certain source of friction from amongst the cogs of your mental machinery.

The following gives the reason for his resigning the Fullerian lectureship:—

ATHENÆUM CLUB, *May*, 1867.

MY DEAR TYNDALL—A conversation I had with Bence Jones yesterday reminded me that I ought to have communicated with you. But we do not meet so often as we used to do, being, I suppose, both very busy, and I forget to write.

You recollect that the last time we talked together, you mentioned a notion of Bence Jones's to make the Fullerian Professorship of Physiology a practically permanent appointment, and that I was quite inclined to stick by that (if such arrangement could be carried out), and give up other things.

But since I have been engaged in the present course of lectures I have found reason to change my views. It is very hard work, and takes up every atom of my time to make the lectures what they should be; and I find that at this time of year, being more or less used up, I suppose, with the winter work, I stand the worry and excitement of the actual lectures very badly. Add to this that it is six weeks clean gone out of the only time I have disposable for real scientific progress, and you will understand how it is that I have made up my mind to resign.

I put all this clearly before Bence Jones yesterday, with the proviso that I could and would do nothing that should embarrass the Institution or himself.

If there is the least difficulty in supplying my place, or if the managers think I shall deal shadily with them by resigning before the expiration of my term, of course I go on. And I hope you all understand that I would do anything rather than put even the appearance of a slight upon those who were kind enough to elect me.—Ever yours,

T. H. HUXLEY.

He found a substitute for 1868, the last year of the triennial course, in Dr. (now Sir) Michael Foster. Of his final lectures in 1867 he used to tell a story against himself.

In my early period as a lecturer, I had very little confidence in my general powers, but one thing I prided myself upon was clearness. I was once talking of the brain before a large mixed audience, and soon began to feel that no one in the room understood me. Finally I saw the thoroughly interested face of a woman auditor, and took consolation in delivering the remainder of the lecture directly to her. At the close, my feeling as to her interest was confirmed when she came up and asked if she might put one question upon a single point which she had not quite understood. "Certainly," I replied. "Now, Professor," she said, "is the cerebellum inside or outside the skull?" (*Reminiscences of T. H. Huxley*, by Professor H. Fairfield Osborn).

Dr. Foster used to add maliciously, that disgust at the small impression he seemed to have made was the true reason for the transference of the lectures.

CHAPTER XXII

1868

IN 1868 he published five scientific memoirs, amongst them his classification of birds and "Remarks upon *Archæopteryx Lithographica*" (*Proc. Roy. Soc.* xvi. 1868, pp. 243-248). This creature, a bird with reptilian characters, was a suggestive object from which to popularise some of the far-reaching results of his many years' labour upon the morphology of both birds and reptiles. Thus it led to a lecture at the Royal Institution, on February 7, "On the Animals which are most nearly intermediate between Birds and Reptiles."

Of this branch of work Sir M. Foster says: (Obit. Not. *Proc. Roy. Soc.* vol. lix.):—

One great consequence of these researches was that science was enriched by a clear demonstration of the many and close affinities between reptiles and birds, so that the two henceforward came to be known under the joint title of Sauropsida, the amphibia being at the same time distinctly more separated from the reptiles, and their relations to fishes more clearly signified by the joint title of Ichthyopsida. At the same time, proof was brought forward that the line of descent of the Sauropsida clearly diverged from that of the Mammalia, both starting from some common ancestry. And besides this great generalisation, the importance of which, both from a classificatory and from an evolutionary point of view, needs no comment, there came out of the same researches numerous lesser contributions to the advancement of morphological knowledge, including among others an attempt, in many respects successful, at a classification of birds.

This work in connection with the reptilian ancestry of birds further appears in the paleontological papers published

But it must be noted that the specimens examined by him and by Haeckel, who two years later published a full and detailed description of *Bathybius*, were seen in a preserved state. Neither of them saw a fresh specimen, though on the cruise of the *Porcupine*, Sir Wyville Thomson and Dr. W. Carpenter examined the substance in a fresh state, and found no better explanation to give of it. However, not only were the expectations that it was very widely distributed over the Atlantic bottom, falsified in 1879 by the researches of the *Challenger* expedition, but the behaviour of certain deep-sea specimens gave good ground for suspecting that what had been sent home before as genuine deep-sea mud, was a precipitate due to the action on the specimens of the spirit in which they were preserved. Though Haeckel, with his special experience of *Monera*, refused to desert *Bathybius*, a close parallel to which was found off Greenland in 1876, the rest of its sponsors gave it up. Whatever it might be as a matter of possibility, the particular evidence upon which it had been described was tainted. Once assured of this, Huxley characteristically took the bull by the horns. Without waiting for any one else to come forward, he made public renunciation of *Bathybius* at the British Association in 1879.* The "eating of the leek" as recommended to his friend Dohrn (July 7, 1868), was not merely a counsel for others, but was a prescription followed by himself on occasion:—

"As you know, I did not think you were on the right track with the Arthropoda, and I am not going to profess to be sorry that you have finally worked yourself to that conclusion.

As to the unlucky publication in the *Journal of Anatomy and Physiology*, you have read your Shakespeare and know what is meant by "eating a leek." Well, every honest man has to do that now and then, and I assure you that if eaten fairly and without grimaces, the devouring of that herb has a very wholesome cooling effect on the blood, particularly in people of sanguine temperament.

Seriously you must not mind a check of this kind.

* See vol. ii. p. 5, sq.

divorced, were closely united; and literature owes him a debt for importing into it so much of the highest scientific habit of mind; for showing that truthfulness need not be bald, and that real power lies more in exact accuracy than in luxuriance of diction. Years after, no less an authority than Spedding, in a letter upon the influence of Bacon on his own style in the matter of exactitude, the pruning of fine epithets and sweeping statements, the reduction of numberless superlatives to positives, asserted that, if as a young man he had fallen in with Huxley's writings before Bacon's, they would have produced the same effect upon him.*

Of the other two discourses referred to, one is the opening address which he delivered as Principal at the South London Working Men's College on January 4, "A Liberal Education, and Where to Find It." This is not a brief for science to the exclusion of other teaching; no essay has insisted more strenuously on the evils of a one-sided education, whether it be classical or scientific; but it urged the necessity for a strong tincture of science and her method, if the modern conception of the world, created by the spread of natural knowledge, is to be fairly understood. If culture is the "criticism of life," it is fallacious if deprived of knowledge of the most important factor which has transformed the medieval into the modern spirit.

Two of his most striking passages are to be found in this address; one the simile of the force behind nature as the hidden chess player; the other the noble description of the end of a true education.

Well known as it is, I venture to quote the latter as an instance of his style:—

That man, I think, has had a liberal education, who has been so trained in youth that his body is the ready servant of his will, and does with ease and pleasure all the work that as a mechanism it is capable of; whose intellect is a clear cold logic engine, with all its parts of equal strength, and in smooth working order; ready, like a steam engine, to be turned to any kind of work, and spin the gossamers as well as forge the anchors of the mind; whose mind is stored with a knowledge of the great

* See p. 520.

the system of M. Comte, he launched his well-known definition of Comtism as Catholicism *minus* Christianity, which involved him in a short controversy with Mr. Congreve (see "The Scientific Aspects of Positivism," *Lay Sermons*, p. 162), and with another leading Positivist, who sent him a letter through Mr. Darwin. Huxley replied:—

JERMYN STREET, *March 11, 1869.*

MY DEAR DARWIN—I know quite enough of Mr. — to have paid every attention to what he has to say, even if you had not been his ambassador.

I glanced over his letter when I returned home last night very tired with my two nights' chairmanship at the Ethnological and the Geological Societies.

Most of it is fair enough, though I must say not helping me to any novel considerations.

Two paragraphs, however, contained opinions which Mr. — is at perfect liberty to entertain, but not, I think, to express to me.

The one is, that I shaped what I had to say at Edinburgh with a view of stirring up the prejudices of the Scotch Presbyterians (imagine how many Presbyterians I had in my audiences!) against Comte.

The other is the concluding paragraph, in which Mr. — recommends me to "*read Comte*," clearly implying that I have criticised Comte without reading him.

You will know how far I am likely to have committed either of the immoralities thus laid to my charge.

At any rate, I do not think I care to enter into more direct relations with anyone who so heedlessly and unjustifiably assumes me to be guilty of them. Therefore I shall content myself with acknowledging the receipt of Mr. —'s letter through you.—Ever yours faithfully,

T. H. HUXLEY.

JERMYN STREET, *March 17, 1869.*

MY DEAR DARWIN—After I had sent my letter to you the other day I thought how stupid I had been not to put in a slip of paper to say it was meant for —'s edification.

I made sure you would understand that I wished it to be sent on, and wrote it (standing on the points of my toes and with my tail up very stiff) with that end in view.

[Sketch of two dogs bristling up.]

I am getting so weary of people writing to propose con-

Comte in his later days was an apostate from his own creed ; his "nouveau grand Être suprême" being as big a fetish as ever nigger first made and then worshipped.—Ever yours faithfully,
T. H. HUXLEY.

It is interesting to note how he invariably submitted his writings to the criticism of his wife before they were seen by any other eye. To her judgment was due the toning down of many a passage which erred by excess of vigour, and the clearing up of phrases which would be obscure to the public. In fact, if an essay met with her approval, he felt sure it would not fail of its effect when published. Writing to her from Norwich on August 23, 1868, he confesses himself with reference to the lecture "On a Piece of Chalk"—

I met Grove who edits *Macmillan*, at the soirée. He pulled the proof of my lecture out of his pocket and said, "Look here, there is one paragraph in your lecture I can make neither top nor tail of. I can't understand what it means." I looked to where his finger pointed, and behold it was the paragraph you objected to when I read you the lecture on the sea shore! I told him; and said I should confess, however set up it might make you.

At the beginning of September, he rejoined his wife and family at Littlehampton, "a grand place for children, because you go *up* rather than *down* into the sea, and it is quite impossible for them to get into mischief by falling," as he described it to his friend Dr. Dohrn, who came down for ten days, eagerly looking forward "to stimulating walks over stock and stone, to Tennyson, Herbert Spencer, and Harry's ringing laugh."

The latter half of the month he spent at or near Dublin, serving upon the Commission on Science and Art Instruction :—

To-day (he writes on September 16), we shall be occupied in inspecting the School of Science and the Glasnevin botanical and agricultural gardens, and to-morrow we begin the session work of examining all the Irishry, who want jobs perpetrated. It is weary work, and the papers are already beginning to tell lies about us and attack us.

will do for it and at the same time (2) for whom it will do. Now you fulfil the first condition admirably, but as to the second I have very great doubts.

In the first place the climate of Calcutta is not particularly good for anyone who has a tendency to dysentery, and I doubt very much if you would stand it for six months.

Secondly, we have a proverb that it is not wise to use razors to cut blocks.

The business of the man who is appointed to that museum will be to get it into order. If he does his duty he will give his time and attention to museum work pure and simple, and I don't think that (especially in an Indian climate), he has much energy left for anything else after the day's work is done. Naming and arranging specimens is a most admirable and useful employment, but when you have done it is "cutting blocks," and you, my friend, are a most indubitable razor, and I do not wish to have your edge blunted in that fashion.

If it were necessary for you to win your own bread, one's advice might be modified. Under such circumstances one must do things which are not entirely desirable. But for you who are your own master and have a career before you, to bind yourself down to work six hours a day at things you do not care about and which others could do just as well, while you are neglecting the things which you do care for, and which others could not do so well, would, I think, be amazingly unwise.

Liberavi animam! don't tell my Indian friends I have dissuaded you, but on my conscience I could give no other advice.

We have to thank you three times over. In the first place for a portrait which has taken its place among those of our other friends; secondly for the great pleasure you gave my little daughter Jessie, by the books you so kindly sent; and thirdly, for Fanny Lewald's autobiography which arrived a few days ago.

Jessie is meditating a letter of thanks (a serious undertaking), and when it is sent the mother will have a word to say for herself.

In the middle of October scarlet fever broke out among my children, and they have all had it in succession, except Jessie, who took it seven years ago. The last convalescent is now well, but we had the disease in the house nearly three months, and have been like lepers, cut off from all communication with our neighbours for that time.

We have had a great deal of anxiety, and my wife has been

An undated note to Darwin belongs to the very end of this year, or to the beginning of the next:—

The two volumes of the new book have just reached me. My best thanks for them; and if you can only send me a little time for reading within the next three months you will heighten the obligation twenty-fold. I wish I had either two heads or a body that needed no rest!

himself would come in upon us at dinner, and patting him on the head, utter what has become a household word amongst us, "Make yourself at home, and take large mouthfuls."

CHAPTER XXIII

1869

IN 1869 Huxley published five paleontological papers, chiefly upon the Dinosaurs (see letter above to Haeckel, January 21, 1868). His physiological researches upon the development of parts of the skull, are represented by a paper for the Zoological Society, while the *Introduction to the Classification of Animals* was a reprint this year of the substance of six lectures in the first part of the lectures on *Elementary Comparative Anatomy* (1864), which were out of print, but still in demand by students.

As President of the Ethnological Society, he delivered an inaugural address "On the Ethnology and Archaeology of India," on March 9, and another "On the Ethnology and Archaeology of North America," on April 13. As president of the Society, moreover, he urged upon the Government the advisability of forming a systematic series of photographs of the various races comprehended in the British Empire, and was officially called upon to offer suggestions for carrying out the project. This appears to be an amplification of Sir Joseph Fayrer's plan in 1866, with respect to all the tribes of India (see p. 294, and Appendix I.).

On April 7 he delivered his "Scientific Education: Notes of an After-Dinner Speech" before the Philomathic Society at Liverpool (*Coll. Ess.* iii. 3), one part of which deals with the attitude of the clergy towards physical science, and expresses the necessary antagonism between science and Roman Catholic doctrine which appears more forcibly in one of his speeches at the School Board in 1871 (see p. 384).

native lines of study according to their tastes and capacities ; but of the earlier part, which was to be obligatory upon all, the report says :—These four years study, if properly employed by the teachers, will constitute a complete preparatory scientific course. However slight the knowledge of details conferred, a wise teacher of any of these subjects will be able to make that teaching thorough ; and to give the scholar a notion of the methods and of the ideas which he will meet with in his further progress in all branches of physical science.

In fact, the fundamental principle was to begin with Observational Science, facts collected ; to proceed to Classificatory Science, facts arranged ; and to end with Inductive Science, facts reasoned upon and laws deduced.

While he was much occupied with the theoretical and practical difficulties of such a scheme of science teaching for general use, he was asked by his friend, the Rev. W. Rogers of Bishopsgate, if he would not deliver a course of lectures on elementary science to boys of the schools in which the latter was interested.

He finally accepted in the following letter, and as the result, delivered twelve lectures week by week from April to June to a large audience at the London Institution in Finsbury Circus, lectures not easily forgotten by the children who listened to them nor by their elders :—

JERMYN STREET, *Feb.* 5, 1869.

MY DEAR ROGERS—Upon due reflection I am not indisposed to undertake the course of lessons we talked about the other day, though they will cost me a good deal of trouble in various ways, and at a time of the year when I am getting to the end of my tether and don't much like trouble.

But the scheme is too completely in harmony with what (in conjunction with Tyndall and others) I have been trying to bring about in schools in general—not to render it a great temptation to me to try to get it into practical shape.

All I have to stipulate is that we shall have a clear understanding on the part of the boys and teachers that the discourses are to [be] *Lessons* and not talkee-talkee lectures. I should like it to be understood that the boys are to take notes and to be examined at the end of the course. Of course I cannot

and uttered a warning against the possible degradation of “a proper reverence for mathematical certainty” into “a superstitious respect for all arguments arrived at by process of mathematics.” *

At the close of the year, as his own period of office came to an end, it was necessary to select a new president of the Geological. He strongly urged Professor (afterwards Sir Joseph) Prestwich to stand, and when the latter consented, a few weeks, by the way, before his marriage was to take place, replied:—

JERMYN STREET, Dec. 16, 1869.

MY DEAR PRESTWICH—Many thanks for your letter. Your consent to become our President for the next period will give as unfeigned satisfaction to the whole body of the Society as it does to me and your other personal friends.

I have looked upon the affair as settled since our last talk, and a very great relief it has been to my mind.

There is no doubt public-dinner speaking (and indeed all public speaking) is nervous work. I funk horribly, though I never get the least credit for it. But it is like swimming, the worst of it is in the first plunge; and after you have taken your “header” it’s not so bad (just like matrimony, by the way; only don’t be so mean as to go and tell a certain lady I said so, because I want to stand well in her books).

Of course you may command me in all ways in which I can possibly be of use. But as one of the chiefs of the Society, and personally and scientifically popular with the whole body, you start with an immense advantage over me, and will find no difficulties before you.

We will consider this business formally settled, and I shall speak of it officially.—Ever yours very faithfully,

T. H. HUXLEY.

I cannot place the following letter to Matthew Arnold with certainty, but it must have been written about this period.† Everyone will sympathise with the situation:—

* See *Coll. Ess.* viii. Introd. p. 8.

† The most probable date being 1869, for on July 1 of that year he dined with Matthew Arnold at Harrow.

JERMYN STREET, *June* 7, 1869.

Private, Confidential, Particular.

MY DEAR FLOWER—I have written to Quain * to tell him that I do not propose to be put in nomination for the Hunterian Chair this year. I really cannot stand it with the British Association hanging over my head. So make thy shoulders ready for the gown, and practise the goose-step in order to march properly behind the mace, and I will come and hear your inaugural.—
Ever yours, T. H. HUXLEY.

The meeting of the Association to which he refers took place at Exeter, and he writes of it to Darwin (September 28):—

As usual, your abominable heresies were the means of getting me into all sorts of hot water at the Association. Three parsons set upon you, and if you were the most malicious of men you could not have wished them to have made greater fools of themselves than they did. They got considerably chaffed, and that was all they were worth.†

And to Tyndall, whom an accident had kept in Switzerland:—

After a sharp fight for Edinburgh, Liverpool was adopted as the place of meeting for the Association of 1870, and I am to be President; although the *Times* says that my best friends tremble for me. (I hope you are not among that particular lot of my best friends.)

I think we shall have a good meeting, and you know you are pledged to give a lecture even if you come with your leg in a sling.

The foundation of the Metaphysical Society in 1869 was not without interest as a sign of the times. As in the new birth of thought which put a period to the Middle Ages, so in the Victorian Renaissance, a vast intellectual ferment had taken immediate shape in a fierce struggle with long

* President of the Royal College of Surgeons.

† It is perhaps scarcely worth while exhuming these long-forgotten arguments in their entirety; but anyone curious enough to consult the report of the meeting preserved in the files of the *Academy*, will find, among other things, an entirely novel theory as to the relation of the Cherubim to terrestrial creation.

of a teacher rather than an iconoclast. The question began to be not whether such opinions are wicked, but whether from the point of view of scientific method they are irrefragably true.

The net philosophical result of the society's work was to distinguish the essential and the unessential differences between the opposite parties; the latter were to a great extent cleared up; but the former remained all the more clearly defined in logical nakedness for the removal of the side issues and the personal idiosyncrasies which often obscured the main issues. Indeed, when this point was reached by both parties, when the origins and consequences of the fundamental principles on either side had been fully discussed and mutual misunderstandings removed to the utmost, so that only the fundamentals themselves remained in debate, there was nothing left to be done. The society, in fact, as Huxley expressed it, "died of too much love."

Indeed, it is to be noticed that, despite the strong antagonism of principle and deductions from principle which existed among the members, the rule of mutual toleration was well kept. The state of feeling after ten years' open struggle seemed likely to produce active collision between representatives of the opposing schools at close quarters. "We all thought it would be a case of Kilkenny cats," said Huxley many years afterwards. "Hats and coats would be left in the hall, but there would be no owners left to put them on again." But only one flash of the sort was elicited. One of the speakers at an early meeting insisted on the necessity of avoiding anything like moral disapprobation in the debates. There was a pause; then W. G. Ward said: "While acquiescing in this condition as a general rule, I think it cannot be expected that Christian thinkers shall give no sign of the horror with which they would view the spread of such extreme opinions as those advocated by Mr. Huxley." Another pause; then Huxley, thus challenged, replied: "As Dr. Ward has spoken, I must in fairness say that it will be very difficult for me to conceal my feeling as to the intellectual degradation which

which can only be called purposive, such as moving to recover its balance when the board on which it stands is inclined, or scratching where it is made uncomfortable, or croaking when pressed in a particular spot. If its spinal cord be severed, the lower limbs, disconnected from the brain, will also perform actions of this kind. The question arises, Is the frog entirely a soulless automaton, performing all its actions directly in response to external stimuli, only more perfectly and with more delicate adjustment when its brain remains intact, or is its soul distributed along its spinal marrow, so that it can be divided into two parts independent of one another?

The professed metaphysician might perhaps tend to regard such consideration as irrelevant; but if the starting-point of metaphysics is to be found in psychology, psychology itself depends to no small extent upon physiology. This question, however, Huxley did not pretend to solve. In the existing state of knowledge he believed it to be insoluble. But he thought it was not without its bearing upon the supposed relations of soul and body in the human subject, and should serve to give pause to current theories on the matter.

His third paper, read January 11, 1876, was on the "Evidence of the Miracle of the Resurrection," in which he argued that there was no valid evidence of actual death having taken place. His rejection of the miraculous had led to an invitation from some of his opponents in the society to write a paper on a definite miracle, and explain his reasons for not accepting it. His choice of subject was due to two reasons: firstly, it was a cardinal instance; secondly, it was a miracle not worked by Christ Himself, and therefore a discussion of its genuineness could offer no suggestion of personal fraud, and hence would avoid inflicting gratuitous pain upon believers in it.

This certainty that there exist many questions at present insoluble, upon which it is intellectually, and indeed morally wrong to assert that we have real knowledge, had long been with him, but, although he had earned abundant odium by openly resisting the claims of dogmatic authority, he had

to be the appropriate title of "agnostic." It came into my head as suggestively antithetic to the "gnostic" of Church history, who professed to know so much about the very things of which I was ignorant; and I took the earliest opportunity of parading it at our Society, to show that I, too, had a tail, like the other foxes. To my great satisfaction, the term took; and when the *Spectator* had stood godfather to it, any suspicion in the minds of respectable people that a knowledge of its parentage might have awakened was, of course, completely lulled.

As for the dialectical powers he displayed in the debates, it was generally acknowledged that in this, as well as in the power of conducting a debate, he shared the pre-eminence with W. G. Ward. Indeed, a proposal was made that the perpetual presidency in alternate years should be vested in these two; but time and health forbade.

His part in the debates is thus described in a letter to me from Professor Henry Sidgwick:—

DEAR MR. HUXLEY—I became a member of the Metaphysical Society, I think, at its first meeting in 1869; and, though my engagements in Cambridge did not allow me to attend regularly, I retain a very distinct recollection of the part taken by your father in the debates at which we were present together. There were several members of the Society with whose philosophical views I had, on the whole, more sympathy; but there was certainly no one to whom I found it more pleasant and more instructive to listen. Indeed I soon came to the conclusion that there was only one other member of our Society who could be placed on a par with him as a debater, on the subjects discussed at our meetings; and that was, curiously enough, a man of the most diametrically opposite opinions—W. G. Ward, the well-known advocate of Ultramontaniam. Ward was by training, and perhaps by nature, more of a dialectician; but your father was unrivalled in the clearness, precision, succinctness, and point of his statements, in his complete and ready grasp of his own system of philosophical thought, and the quickness and versatility with which his thought at once assumed the right attitude of defence against any argument coming from any quarter. I used to think that while others of us could perhaps find, on the spur of the moment, *an* answer more or less effective to some unexpected attack, your father seemed always able to find *the* answer—I mean the answer that it was reasonable to

give, consistently with his general view, and much the same answer that he would have given if he had been allowed the fullest time for deliberation.

The general tone of the Metaphysical Society was one of extreme consideration for the feelings of opponents, and your father's speaking formed no exception to the general harmony. At the same time I seem to remember him as the most combative of all the speakers who took a leading part in the debates. His habit of never wasting words, and the edge naturally given to his remarks by his genius for clear and effective statement, partly account for this impression; still I used to think that he liked fighting, and occasionally liked to give play to his sarcastic humour—though always strictly within the limits imposed by courtesy. I remember that on one occasion when I had read to the Society an essay on the "Incoherence of Empiricism," I looked forward with some little anxiety to his criticisms; and when they came, I felt that my anxiety had not been superfluous; he "went for" the weak points of my argument in half a dozen trenchant sentences, of which I shall not forget the impression. It was hard hitting, though perfectly courteous and fair.

I wish I could remember what he said, but the memory of all the words uttered in these debates has now vanished from my mind, though I recall vividly the general impression that I have tried briefly to put down.—Believe me, yours very truly,

HENRY SIDGWICK.

CHAPTER XXIV

1870

WITH the year 1870 comes another turning-point in Huxley's career. From his return to England in 1850 till 1854 he had endured four years of hard struggle, of hope deferred; his reputation as a zoologist had been established before his arrival, and was more than confirmed by his personal energy and power. When at length settled in the professorship at Jermyn Street, he was so far from thinking himself more than a beginner who had learned to work in one corner of the field of knowledge, still needing deep research into all kindred subjects in order to know the true bearings of his own little portion, that he treated the next six years simply as years of further apprenticeship. Under the suggestive power of the *Origin of Species* all these scattered studies fell suddenly into due rank and order; the philosophic unity he had so long been seeking inspired his thought with tenfold vigour, and the battle at Oxford in defence of the new hypothesis first brought him before the public eye as one who not only had the courage of his convictions when attacked, but could, and more, would, carry the war effectively into the enemy's country. And for the next ten years he was commonly identified with the championship of the most unpopular view of the time; a fighter, an assailant of long-established fallacies, he was too often considered a mere iconoclast, a subverter of every other well-rooted institution, theological, educational, or moral.

It is difficult now to realise with what feelings he was regarded in the average respectable household in the sixties and early seventies. His name was anathema; he was a

the popular mind the leaven of new ideas upon nature and education and the progress of thought, he was still constantly at work on biological researches of his own, many of which took shape in the Hunterian lectures at the College of Surgeons from 1863-1870. But from 1870 onward, the time he would spare to such research grew less and less. For eight years he was continuously on one Royal Commission after another. His administrative work on learned societies continued to increase; in 1869-70 he held the presidency of the Ethnological Society, with a view to effecting the amalgamation with the Anthropological, "the plan," as he calls it, "for uniting the Societies which occupy themselves with man (that excludes "Society" which occupies itself chiefly with woman)." He became president of the Geological Society in 1872, and for nearly ten years, from 1871 to 1880, he was secretary of the Royal Society, an office which occupied no small portion of his time and thought, "for he had formed a very high ideal of the duties of the Society as the head of science in this country, and was determined that it should not at least fall short through any lack of exertion on his part" (Sir M. Foster, R. S. Obit. Not.).*

The year 1870 itself was one of the busiest he had ever known. He published one biological and four paleontological memoirs, and sat on two Royal Commissions, one on the Contagious Diseases Acts, the other on Scientific Instruction, which continued until 1875.

The three addresses which he gave in the autumn, and his election to the School Board will be spoken of later: in the first part of the year he read two papers at the Ethnological Society, of which he was president, on "The Geographical Distribution of the Chief Modifications of Man," March 9—and on "The Ethnology of Britain," May 22—the substance of which appeared in the *Contemporary Review* for July under the title of "Some Fixed Points in British Ethnology" (*Cont. Rev.* vii. 253). As president also of the Geological Society and of the British Association.

* See Appendix II.

est of which, a parody of Sydney Smith's dictum on Dr. Whewell, Huxley replied:—

"A Devonshire Man" is good enough to say of me that "cutting up monkeys is his forte, and cutting up men is his foible." With your permission, I propose to cut up "A Devonshire Man"; but I leave it to the public to judge whether, when so employed, my occupation is to be referred to the former or to the latter category.

For this he was roundly lectured by the *Spectator* on January 29, in an article under the heading "Pope Huxley." Regardless of the rights or wrongs of the controversy, he was chidden for the abusive language of the above paragraph, and told that he was a very good anatomist, but had better not enter into discussions on other subjects.

The same question is developed in the address to the Ethnological Society later in the year and in "Some Fixed Points in British Ethnology" (*Contemporary Review*, 1870), and reiterated in an address from the chair in Section D at the British Association in 1878 at Dublin, and in a letter to the *Times* for October 12, 1887, apropos of a leading article upon "British Race-types of To-day." (See i., 284.)

Letter-writing was difficult under such pressure of work, but the claims of absent friends were not wholly forgotten, though left on one side for a time, and the warm-hearted Dohrn, who could not bear to think himself forgotten, managed to get a letter out of him—not on scientific business.

26 ABBEY PLACE, Jan. 30, 1870.

MY DEAR DOHRN—In one sense I deserve all the hard things you may have said and thought about me, for it is really scandalous and indefensible that I have not written to you. But in another sense, I do not, for I have very often thought about you and your doings, and as I have told you once before, your memory always remains green in the "happy family."

But what between the incessant pressure of work and an inborn aversion to letter-writing, I become a worse and worse correspondent the longer I live, and unless I can find one or two friends who will [be] content to bear with my infirmities and believe that however long before we meet, I shall be ready to take them up again exactly where I left off, I shall be a friendless old man,

If (he said) the expectation raised by the splints of the horses that, in some ancestor of the horses, these splints would be found to be complete digits, has been verified, we are furnished with very strong reasons for looking for a no less complete verification of the expectation that the three-toed *Plagiolophus*-like "avus" of the horse must have been a five-toed "atavus" at some early period.

Six years afterwards, this forecast of paleontological research was to be fulfilled, but at the expense of the European ancestry of the horse. A series of ancestors, similar to these European fossils, but still more equine, and extending in unbroken order much farther back in geological time, was discovered in America. His use of this in his New York lectures as demonstrative evidence of evolution, and the immediate fulfilment of a further prophecy of his will be told in due course.

His address to the Cambridge Y.M.C.A., "A Commentary on Descartes' 'Discourse touching the method of using reason rightly, and of seeking scientific truth,'" was delivered on March 24. This was an attempt to give this distinctively Christian audience some vision of the world of science and philosophy, which is neither Christian nor Unchristian, but Extra-christian, and to show "by what methods the dwellers therein try to distinguish truth from falsehood, in regard to some of the deepest and most difficult problems that beset humanity, "in order to be clear about their actions, and to walk sure-footedly in this life," as Descartes says. For Descartes had laid the foundation of his own guiding principle of "active scepticism, which strives to conquer itself."

Here again, as in the *Physical Basis of Life*, but with more detail, he explains how far materialism is legitimate, is, in fact, a sort of shorthand idealism. This essay, too, contains the often-quoted passage, apropos of the "introduction of Calvinism into science."

I protest that if some great Power would agree to make me always think what is true and do what is right, on condition of being turned into a sort of clock and wound up every morning before I got out of bed, I should instantly close with the offer.

be carried out before his lectures on electricity at the end of the month. So he writes on April 6:—

ROYAL INSTITUTION, 6 *April*.

MY DEAR HUXLEY—I was rendered drunk by the excess of prospective pleasure when you mentioned the Eifel yesterday, and took no account of my lectures. They begin on the 28th, and I have studiously to this hour excluded them from my thought. I have made arrangements to see various experiments involving the practical application of electricity before the lectures begin; I find myself, in short, cut off from the expedition. My regret on this score is commensurable with the pleasures I promised myself. Confound the lectures!

And yours * on Friday is creating a pretty hubbub already. I am torn to pieces by women in search of tickets. Anything that touches progenitorship interests them. You will have a crammed house I doubt not.—Yours ever,

JOHN TYNDALL.

Huxley replied:—

GEOLOGICAL SURVEY OF ENGLAND AND WALES,
April 6, 1870.

MY DEAR TYNDALL—
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T. H. H.

That's a practical application of electricity for you.

In June he writes to his wife, who had taken a sick child to the seaside:—

I hear a curious rumour (which is not for circulation), that Froude and I have been proposed for D.C.L.'s at Commemoration, and that the proposition has been bitterly and strongly opposed by Pusey.† They say there has been a regular row in

* *On the Pedigree of the Horse*, April 8, 1870, which was never brought out in book form.

† Huxley ultimately received his D.C.L. in 1885.

ogy * led him to carry on a series of investigations lasting over two years, which took shape in a paper upon "Penicillium, Torula, and Bacterium," † first read in Section D at the British Association, 1870; and in his article on "Yeast" in the *Contemporary Review* for December 1872. He laboriously repeated Pasteur's experiments, and for years a quantity of flasks and cultures used in this work remained at South Kensington, until they were destroyed in the eighties. Of this work Sir J. Hooker writes to him:—

You have made an immense leap in the association of forms, and I cannot but suppose you approach the final solution. . . .

I have always fancied that it was rather brains and boldness, than eyes or microscopes that the mycologists wanted, and that there was more brains in Berkeley's ‡ crude discoveries than in the very best of the French and German microscopic verifications of them, who filch away the credit of them from under Berkeley's nose, and pooh-pooh his reasoning, but for which we should be, as we were.

In his Presidential Address, "Biogenesis and Abiogenesis" (*Coll. Ess.* viii. p. 229), he discussed the rival theories of spontaneous generation and the universal derivation of life from precedent life, and professed his belief, as an act of philosophic faith, that at some remote period, life had arisen out of inanimate matter, though there was no evidence that anything of the sort had occurred recently, the germ theory explaining many supposed cases of spontaneous generation. The history of the subject, indeed, showed "the great tragedy of Science—the slaying of a beautiful hypothesis by an ugly fact—which is so constantly being enacted under the eyes of philosophers," and recalled the warning "that it is one thing to refute a proposition, and another to prove the truth of a doctrine which, implicitly or explicitly, contradicts that proposition."

Two letters to Dr. Dohrn refer to this address and to the meeting of the Association.

* See p. 405, *sqq.* † *Quart. Journ. Micr. Sci.*, 1870, x. pp. 355–362.

‡ Rev. W. F. Berkeley.

Now I think that the best service I can render to all you enterprising young men is to turn devil's advocate, and do my best to pick holes in your work.

By the way Mikluko-Maclay * has been here; I have seen a good deal of him, and he strikes me as a man of very considerable capacity and energy. He was to return to Jena to-day.

My friend Herbert Spencer will be glad to learn that you appreciate his book. I have been *his* devil's advocate for a number of years, and there is no telling how many brilliant speculations I have been the means of choking in an embryonic state.

My wife does not know that I am writing to you, or she would say apropos of your last paragraph that you are an entirely unreasonable creature in your notions of how friendship should be manifested, and that you make no allowances for the oppression and exhaustion of the work entailed by what Jean Paul calls a "Töchtervolles Haus." I hope I may live to see you with at least ten children, and then my wife and I will be avenged. Our children will be married and settled by that time, and we shall have time to write every day and get very wroth when you do not reply immediately.—Ever yours faithfully,

T. H. HUXLEY.

All are well, the children so grown you will not know them.

July 18, 1870.

MY DEAR DOHRN—Notwithstanding the severe symptoms of "Töchterkrankheit" under which I labour, I find myself equal to reply to your letter.

The British Association meets in September on the 14th day of that month, which falls on a Wednesday. Of course, if you come you shall be provided for by the best specimen of Liverpool hospitality. We have ample provision for the entertainment of the "distinguished foreigner."

Will you be so good as to be my special ambassador with Haeckel and Gegenbaur, and tell them the same thing? It would give me and all of us particular pleasure to see them and to take care of them.

But I am afraid that this wretched war will play the very deuce with our foreign friends. If you Germans do not give

* Mikluko-Maclay, a Russian naturalist, and close friend of Haeckel's, who later adventured himself alone among the cannibals of New Guinea.

On the 21st, after the general meeting of the Association, which wound up the proceedings, the Historical Society of Lancashire and Cheshire presented a diploma of honorary membership and a gift of books to Huxley, Sir G. Stokes, and Sir J. Hooker, the last three Presidents of the British Association, and to Professors Tyndall and Rankine and Sir J. Lubbock, the lecturers at Liverpool. Then Huxley was presented with a mazer bowl lined with silver, made from part of one of the roof timbers of the cottage occupied as his headquarters by Prince Rupert during the siege of Liverpool. He was rather taken aback when he found the bowl was filled with champagne; after a moment, however, he drank "success to the good old town of Liverpool," and with a wave of his hand, threw the rest on the floor, saying, "I pour this as a libation to the tutelary deities of the town."

The same evening he was the guest of the Sphinx Club at dinner at the Royal Hotel, his friend Mr. P. H. Rathbone being in the chair, and in proposing the toast of the town and trade of Liverpool, declared that commerce was a greater civiliser than all the religion and all the science ever put together in the world, for it taught men to be truthful and punctual and precise in the execution of their engagements, and men who were truthful and punctual and precise in the execution of their engagements had put their feet upon the first rung of the ladder which led to moral and intellectual elevation.

There were the usual clerical attacks on the address, among the rest a particularly violent one from a Unitarian pulpit. Writing to Mr. Samuelson on October 5 he says:—

Be not vexed on account of the godly. They will have their way. I found Mr. ——'s sermon awaiting me on my return home. It is an able paper, but like the rest of his cloth he will not take the trouble to make himself acquainted with the ideas of the man whom he opposes. At least that is the case if he imagines he brings me under the range of his guns.

On October 2 he writes to Tyndall:—

I have not yet thanked you properly for your great contribution to the success of our meeting [*i.e.* his lecture "On the

poned his Lectures to Working Men in London from October to February 1871. On October 3 he lectured in Leicester on "What is to be Learned from a Piece of Coal," a parallel lecture to that of 1868 on "A Piece of Chalk." On the 17th and 24th he lectured at Birmingham on "Extinct Animals intermediate between Reptiles and Birds"—a subject which he had made peculiarly his own by long study; and on December 29 he was at Bradford, and lectured at the Philosophical Institute upon "The Formation of Coal" (*Coll. Ess.* viii.).

He was also busy with two Royal Commissions; still, at whatever cost of the energy and time due to his own investigations and those additional labours by which he increased his none too abundant income, he felt it his duty, in the interests of his ideal of education, to come forward as a candidate for the newly-instituted School Board for London. This was the practical outcome of the rising interest in education all over the country; on its working, he felt, depended momentous issues—the fostering of the moral and physical well-being of the nation; the quickening of its intelligence and the maintenance of its commercial supremacy. Withal, he desired to temper "book-learning" with something of the direct knowledge of nature: on the one hand, as an admirable instrument of education, if properly applied; on the other, as preparing the way for an attitude of mind which could appreciate the reasons for the immense changes already beginning to operate in human thought.

Moreover, he possessed a considerable knowledge of the working of elementary education throughout the country, owing to his experience as examiner under the Science and Art Department, the establishment of which he describes as "a measure which came into existence unnoticed, but which will, I believe, turn out to be of more importance to the welfare of the people than many political changes over which the noise of battle has rent the air" (*Scientific Education*, 1869; *Coll. Ess.* iii. p. 131).

Accordingly, though with health uncertain, and in the midst of exacting occupations, he felt that he ought not to stand aside at so critical a moment, and offered himself for

the science, theology, with the affection, religion, and either crying for more theology under the name of religion, or demanding the abolition of "religious" teaching in order to get rid of theology, a step which he likens to "burning your ship to get rid of the cockroaches."

As regards his actual work on the Board, I must express my thanks to Dr. J. H. Gladstone for his kindness in supplementing my information with an account based partly on his own long experience of the Board, partly on the reminiscences of members contemporary with my father.

The Board met first on December 15, for the purpose of electing a Chairman. As a preliminary, Huxley proposed and carried a motion that no salary be attached to the post. He was himself one of the four members proposed for the Chairmanship; but the choice of the Board fell upon Lord Lawrence. In the words of Dr. Gladstone:—

Huxley at once took a prominent part in the proceedings, and continued to do so till the beginning of the year 1872, when ill-health compelled him to retire.

At first there was much curiosity both inside and outside the Board as to how Huxley would work with the old educationists, the clergy, dissenting ministers, and the miscellaneous body of eminent men that comprised the first Board. His antagonism to many of the methods employed in elementary schools was well known from his various discourses, which had been recently published together under the title of *Lay Sermons, Addresses, and Reviews*. I watched his course with interest at the time; but for the purpose of this sketch I have lately sought information from such of the old members of the Board as are still living, especially the Earl of Harrowby, Bishop Barry, the Rev. Dr. Angus, and Mr. Edward North Buxton, together with Mr. Croad, the Clerk of the Board. They soon found proof of his great energy, and his power of expressing his views in clear and forcible language; but they also found that with all his strong convictions and lofty ideals he was able and willing to enter into the views of others, and to look at a practical question from its several sides. He could construct as well as criticise. Having entered a public arena somewhat late in life, and being of a sensitive nature, he had scarcely acquired that calmness and pachydermatous quality which is needful for one's personal

upon the scheme of education to be adopted in the Board Schools.

This motion came up for consideration on February 15, 1871. In introducing it, he said that such a committee ought to consider—

First, the general nature and relations of the schools which may come under the Board. Secondly, the amount of time to be devoted to educational purposes in such schools; and Thirdly, the subject-matter of the instruction or education, or teaching, or training, which is to be given in these schools.

But this, by itself, he continued, would be incomplete. At one end of the scale he advocated Infant schools, and urged a connection with the excellent work of the Ragged schools. At the other end he desired to see continuation schools, and ultimately some scheme of technical education. A comprehensive scheme, indeed, would involve an educational ladder from the gutter to the university, whereby children of exceptional ability might reach the place for which nature had fitted them.

The subject matter of elementary instruction must be limited by what was practicable and desirable. The revised code had done too little; it had taught the use of the tools of learning, while denying all sorts of knowledge on which to exercise them afterwards. And here incidentally he repudiated the notion that the English child was stupid; on the contrary, he thought the two finest intellects in Europe at this time were the English and the Italian.

In particular he advocated the teaching of "the first elements of physical science"; "by which I do not mean teaching astronomy and the use of the globes, and the rest of the abominable trash—but a little instruction of the child in what is the nature of common things about him; what their properties are, and in what relation this actual body of man stands to the universe outside of it." "There is no form of knowledge or instruction in which children take greater interest."

Drawing and music, too, he considered, should be taught in every elementary school, not to produce painters or

things," which he called the scientific aspect, for want of a better name.

"It had been very justly said that they had a great mass of low half-instructed population which owed what little redemption from ignorance and barbarism it possessed mainly to the efforts of the clergy of the different denominations. Any system of gaining the attention of these people to these matters must be a system connected with, or not too rudely divorced from their own system of belief. He wanted regulations, not in accordance with what he himself thought was right, but in the direction in which thought was moving." He wanted an elastic system, that did not oppose any obstacle to the free play of the public mind.

Huxley voted against all the proposed amendments, and in favour of Mr. Smith's motion. There were only three who voted against it; while the three Roman Catholic members refrained from voting. This basis of religious instruction, practically unaltered, has remained the law of the Board ever since.

There was a controversy in the papers, between Prof. Huxley and the Rev. W. H. Fremantle, as to the nature of the explanations of the Bible lessons. Huxley maintained that it should be purely grammatical, geographical, and historical in its nature; Fremantle that it should include some species of distinct religious teaching, but not of a denominational character.*

In taking up this position, Huxley expressly disclaimed any desire for a mere compromise to smooth over a difficulty. He supported what appeared to be the only workable plan under the circumstances, though it was not his ideal; for he would not have used the Bible as the agency for introducing the religious and ethical idea into education if he had been dealing with a fresh and untouched population.

His appreciation of the literary and historical value of the Bible, and the effect it was likely to produce upon the

* Cp. extract from Lord Shaftesbury's journal about this correspondence (*Life and Work of Lord Shaftesbury*, iii. 282). "Professor Huxley has this definition of morality and religion: 'Teach a child what is wise, that is *morality*. Teach him what is wise and beautiful, that is *religion*!' Let no one henceforth despair of making things clear and of giving explanations!"

The second is to a correspondent who wrote to ask him whether adhesion to the compromise had not rendered nonsensical the teaching given in a certain lesson upon the finding of the youthful Jesus in the temple, when, after they had read the verse, "How is it that ye sought me? Wist ye not that I must be about my Father's business?" the teacher asked the children the name of Jesus' father and mother, and accepted the simple answer, Joseph and Mary. Thus the point of the story, whether regarded as reality or myth, is slurred over, the result is perplexity, the teaching, in short, is bad, apart from all theory as to the value of the Bible.

In a letter to the *Chronicle*, which he forwarded, this correspondent suggested a continuation of the "incriminated lesson":—

Suppose, then, that an intelligent child of seven, who has just heard it read out that Jesus excused Himself to His parents for disappearing for three days, on the ground that He was about His Father's business, and has then learned that His father's name was Joseph, had said "Please, teacher, was this the Jesus that gave us the Lord's Prayer?" The teacher answers "Yes." And suppose the child rejoins, "And is it to His father Joseph that he bids us pray when we say Our Father?" But there are boys of nine, ten, eleven years in Board schools, and many such boys are intelligent enough to take up the subject of the lesson where the instructor left it. "Please, teacher," asks one of these, "what business was it that Jesus had to do for His father Joseph? Had he stopped behind to get a few orders? Was it true that He had been about Joseph's business? And, if it was not true, did He not deserve to be punished?"

Huxley replied on October 16, 1894:—

DEAR SIR—I am one with you in hating "hush up" as I do all other forms of lying; but I venture to submit that the compromise of 1871 was not a "hush-up." If I had taken it to be such I should have refused to have anything to do with it. And more specifically, I said in a letter to the *Times* (see *Times*, 29th April 1893) at the beginning of the present controversy, that if I had thought the compromise involved the obligatory teaching of such dogmas as the Incarnation I should have opposed it.

There has never been the slightest ambiguity about my posi-

the Incarnation as warmly as that of the Trinity), it may be well to leave things as they are.

All this is for your own eye. There is nothing in substance that I have not said publicly, but I do not feel called upon to say it over again, or get mixed up in an utterly wearisome controversy.—I am, yours faithfully,
T. H. HUXLEY.

However, he was unsuccessful in his proposal that a selection be made of passages for reading from the Bible; the Board refused to become censors. On May 10 he raised the question of the diversion from the education of poor children of charitable bequests, which ought to be applied to the augmentation of the school fund. In speaking to this motion he said that the long account of errors and crimes of the Catholic Church was greatly redeemed by the fact that that Church had always borne in mind the education of the poor, and had carried out the great democratic idea that the soul of every man was of the same value in the eyes of his Maker.

The next matter of importance in which he took part was on June 14, when the Committee on the Scheme of Education presented its first report. Dr. Gladstone writes:—

It was a very voluminous document. The Committee had met every week, and, in the words of Huxley, "what it had endeavoured to do, was to obtain some order and system and uniformity in important matters, whilst in comparatively unimportant matters they thought some play should be given for the activity of the bodies of men into whose hands the management of the various schools should be placed." The recommendations were considered on June 21 and July 12, and passed without any material alterations or additions. They were very much the same as existed in the best elementary schools of the period. Huxley's chief interest, it may be surmised, was in the subjects of instruction. It was passed that, in infants' schools there should be the Bible, reading, writing, arithmetic, object lessons of a simple character, with some such exercise of the hands and eyes as is given in the Kindergarten system, music, and drill. In junior and senior schools the subjects of instruction were divided into two classes, essential and discretionary, the essentials being the Bible, and the principles of religion and

Further, although the committee as a whole recommended that discretionary subjects should be extras, he wished them to be covered by the general payment, in which sense the report was amended.

This Education Committee (proceeds Dr. Gladstone) continued to sit, and on November 30 brought up a report in favour of the Prussian system of separate class-rooms, to be tried in one school as an experiment. This reads curiously now that it has become the system almost universally adopted in the London Board Schools.

In regard to examinations Huxley strongly supported the view that the teaching in all subjects, secular or sacred, should be periodically tested.

On December 13, Huxley raised the question whether the selection of books and apparatus should be referred to his Committee or to the School Management Committee, and on January 10 following, a small sub-committee for that object was formed. Almost immediately after this he retired from the Board.

One more speech of his, which created a great stir at the time, must be referred to, namely his expression of undisguised hostility to the system of education maintained by the Ultramontane section of the Roman Catholics.* In October the bye-laws came up for consideration. One of them provided that the Board should pay over direct to denominational schools the fees for poor children. This he opposed on the ground that it would lead to repeated contests on the Board, and further, might be used as a tool by the Ultramontanes for their own purposes. Believing that their system as set forth in the syllabus, of securing complete possession of the minds of those whom they taught or controlled, was destructive to all that was highest in the nature of mankind, and inconsistent with intellectual and political liberty, he considered it his earnest duty to oppose all measures which would lead to assisting the Ultramontanes in their purpose.

Hereupon he was vehemently attacked, for example, in the *Times* for his "injudicious and even reprehensible tone"

* Cp. "Scientific Education," *Coll. Ess.* iii. p. 111.

lectual teaching, but what we principally want is the moral teaching."

As to the sub-committee on books and apparatus, it did little at first, but at the beginning of the second Board, 1873, it became better organised under the presidency of the Rev. Benjamin Waugh. At the commencement of the next triennial term I became the chairman, and continued to be such for eighteen years. It was our duty to put into practice the scheme of instruction which Huxley was mainly instrumental in settling. We were thus able indirectly to improve both the means and methods of teaching. The subjects of instruction have all been retained in the Curriculum of the London School Board, except, perhaps, "mensuration" and "social economy." The most important developments and additions have been in the direction of educating the hand and eye. Kindergarten methods have been promoted. Drawing, on which Huxley laid more stress than his colleagues generally did, has been enormously extended and greatly revolutionised in its methods. Object lessons and elementary science have been introduced everywhere, while shorthand, the use of tools for boys, and cookery and domestic economy for girls are becoming essentials in our schools. Evening continuation schools have lately been widely extended. Thus the impulse given by Huxley in the first months of the Board's existence has been carried forward by others, and is now affecting the minds of the half million of boys and girls in the Board Schools of London, and indirectly the still greater number in other schools throughout the land.

I must further express my thanks to Bishop Barry for permission to make use of the following passages from the notes contributed by him to Dr. Gladstone:—

I had the privilege of being a member of his committee for defining the curriculum of study, and here also—the religious question being disposed of—I was able to follow much the same line as his, and I remember being struck not only with his clear-headed ability, but with his strong commonsense, as to what was useful and practicable, and the utter absence in him of *doctrinaire* aspiration after ideal impossibilities. There was (I think) very little under his chairmanship of strongly accentuated difference of opinion.

In his action on the Board generally I was struck with these three characteristics:—First, his remarkable power of speaking

His contempt for the idea of the world into which we were born being either a sort of clergyhouse or a market-place, was too complete to be marked by any eagerness. But in view of the market-place idea he was the less calm.

Like many others who had not yet come to know in what high esteem he held the moral and spiritual nature of children, I had thought he was the advocate of mere secular studies, alike in the nation's schools, and in its families. But by contact with him, this soon became an impossible idea. In very early days on the Board a remark I had made to a mutual friend which implied this unjust idea was repeated to him. "Tell Waugh that he talks too fast," was his message to me. I was not long in finding out that this was a very just reproof. . . .

The two things in his character of which I became most conscious by contact with him, were his childlikeness and his consideration for intellectual inferiors. His arguments were as transparently honest as the arguments of a child. They might or might not seem wrong to others, but they were never untrue to himself. Whether you agreed with them or not, they always added greatly to the charm of his personality. Whether his face was lighted by his careless and playful humour or his great brows were shadowed by anger, he was alike expressing himself with the honesty of a child. What he counted iniquity he hated, and what he counted righteous he loved with the candour of a child. . . .

Of his consideration for intellectual inferiors I, of course, needed a large share, and it was never wanting. Towering as was his intellectual strength and keenness above me, indeed above the whole of the rest of the members of the Board, he did not condescend to me. The result was never humiliating. It had no pain of any sort in it. He was too spontaneous and liberal with his consideration to seem conscious that he was showing any. There were many men of religious note upon the Board, of some of whom I could not say the same.

In his most trenchant attacks on what he deemed wrong in principles, he never descended to attack either the sects which held them or the individuals who supported them, even though occasionally much provocation was given him. He might not care for peace with some of the theories represented on the Board, but he had certainly and at all times great good-will to men.

As a speaker he was delightful. Few, clear, definite, and calm as stars were the words he spoke. Nobody talked whilst

political career. To one of those who urged him to stand for Parliament, he replied thus:—

Nov. 18, 1871.

DEAR SIR—It has often been suggested to me that I should seek for a seat in the House of Commons; indeed I have reason to think that many persons suppose that I entered the London School Board simply as a road to Parliament.

But I assure you that this supposition is entirely without foundation, and that I have never seriously entertained any notion of the kind.

The work of the School Board involves me in no small sacrifices of various kinds, but I went into it with my eyes open, and with the clear conviction that it was worth while to make those sacrifices for the sake of helping the Education Act into practical operation. A year's experience has not altered that conviction; but now that the most difficult, if not the most important, part of our work is done, I begin to look forward with some anxiety to the time when I shall be relieved of duties which so seriously interfere with what I regard as my proper occupation.

No one can say what the future has in store for him, but at present I know of no inducement, not even the offer of a seat in the House of Commons, which would lead me, even temporarily and partially, to forsake that work again.—I am, dear sir, yours very faithfully,

T. H. HUXLEY.

I give here a letter to me from Sir Mountstuart Grant Duff, who also at one period was anxious to induce him to enter Parliament:—

LEXDEN PARK, COLCHESTER,
4th November 1898.

DEAR MR. HUXLEY—I have met men who seemed to me to possess powers of mind even greater than those of your father—his friend Henry Smith for example; but I never met any one who gave me the impression so much as he did, that he would have gone to the front in any pursuit in which he had seen fit to engage. Henry Smith had, in addition to his astonishing mathematical genius, and his great talents as a scholar, a rare faculty of persuasiveness. Your father used to speak with much admiration and some amusement of the way in which he managed to get people to take his view by appearing to take theirs; but he never could have been a power in a popular assembly, nor have carried with him by the force of his eloquence,

traordinary gifts would have been indeed a rare piece of good fortune, and I should have been able also to have contributed to the work upon which you are engaged a great many facts which would have been of interest to your readers. You will, however, I am sure, take the will for the deed, and believe me, very sincerely yours,

M. E. GRANT DUFF.

schoolmasters—that important business of teaching the teachers that they might set about scientific instruction in the right way.* He attended the British Association at Edinburgh, and laid down his Presidency; he brought out his “Manual of Vertebrate Anatomy,” and wrote a review of “Mr. Darwin’s Critics” (*see* p. 391, *sq.*), while on October 9 he delivered an address at the Midland Institute, Birmingham, on “Administrative Nihilism” (*Coll. Ess.* i.). This address, written between September 21 and 28, and remodelled later, was a pendant to his educational campaign on the School Board; a re-statement and justification of what he had said and done there. His text was the various objections raised to State interference with education; he dealt first with the upholders of a kind of caste system, men who were willing enough to raise themselves and their sons to a higher social plane, but objected on semi-theological grounds to anyone from below doing likewise—neatly satirising them and their notions of gentility, and quoting Plato in support of his contention that what is wanted even more than means to help capacity to rise is “machinery by which to facilitate the descent of incapacity from the higher strata to the lower.” He repeats in new phrase his warning “that every man of high natural ability, who is both ignorant and miserable, is as great a danger to society as a rocket without a stick is to people who fire it. Misery is a match that never goes out; genius, as an explosive power, beats gunpowder hollow: and if knowledge, which should give that power guidance, is wanting, the chances are not small that the rocket will simply run a-muck among friends and foes.”

Another class of objectors will have it that government should be restricted to police functions, both domestic and foreign, that any further interference must do harm.

Suppose, however, for the sake of argument, that we accept the proposition that the functions of the State may be properly summed up in the one great negative commandment—“Thou shalt not allow any man to interfere with the liberty of any

* See pp. 389, 405, *sq.*

Of Mr. Spencer's comparison of the State to a living body in the interests of individualism:—

I suppose it is universally agreed that it would be useless and absurd for the State to attempt to promote friendship and sympathy between man and man directly. But I see no reason why, if it be otherwise expedient, the State may not do something towards that end indirectly. For example, I can conceive the existence of an Established Church which should be a blessing to the community. A Church in which, week by week, services should be devoted, not to the iteration of abstract propositions in theology, but to the setting before men's minds of an ideal of true, just, and pure living; a place in which those who are weary of the burden of daily cares should find a moment's rest in the contemplation of the higher life which is possible for all, though attained by so few; a place in which the man of strife and of business should have time to think how small, after all, are the rewards he covets compared with peace and charity. Depend upon it, if such a Church existed, no one would seek to disestablish it.

The sole order of nobility which, in my judgment, becomes a philosopher, is the rank which he holds in the estimation of his fellow-workers, who are the only competent judges in such matters. Newton and Cuvier lowered themselves when the one accepted an idle knighthood, and the other became a baron of the empire. The great men who went to their graves as Michael Faraday and George Grote seem to me to have understood the dignity of knowledge better when they declined all such meretricious trappings.*

The usual note of high pressure recurs in the following letter, written to thank Darwin for his new work, *The Descent of Man, and Sexual Selection*.

JERMYN STREET, Feb. 20, 1871.

MY DEAR DARWIN—Best thanks for your new book, a copy of which I find awaiting me this morning. But I wish you would not bring your books out when I am so busy with all sorts of things. You know I can't show my face anywhere in society without having read them—and I consider it too bad.

* On the other hand, he thought it right and proper for officials, in scientific as in other departments, to accept such honours, as giving them official power and status. In his own case, while refusing all

jects has, I hope, secured the future of Elementary Science in London. Cannot you get as much done in Manchester?—Ever yours faithfully,
T. H. HUXLEY.

Sir Charles Lyell was now nearly 74 years old, and though he lived four years longer, age was beginning to tell even upon his vigorous powers. A chance meeting with him elicited the following letter:—

26 ABBEY PLACE, *July* 30, 1871.

MY DEAR DARWIN—I met Lyell in Waterloo Place to-day walking with Carrick Moore—and although what you said the other day had prepared me, I was greatly shocked at his appearance, and still more at his speech. There is no doubt it is affected in the way you describe, and the fact gives me very sad forebodings about him. The Fates send me a swift and speedy end whenever my time comes. I think there is nothing so lamentable as the spectacle of the wreck of a once clear and vigorous mind!

I am glad Frank enjoyed his visit to us. He is a great favourite here, and I hope he will understand that he is free of the house. It was the greatest fun to see Jess and Mady* on their dignity with him. No more kissing, I can tell you. Miss Mady was especially sublime.

Six out of our seven children have the whooping-cough. Need I say therefore that the wife is enjoying herself?

With best regards to Mrs. Darwin and your daughter (and affectionate love to Polly) believe me.—Ever yours faithfully,
T. H. HUXLEY.

The purchase of the microscope, already referred to, was the subject of another letter to Dr. Dohrn, of which only the concluding paragraph about the School Board, is of general interest. Unfortunately the English microscope did not turn out a success, as compared to the work of the Jena opticians: this is the “optical Sadowa” of the second letter.

I fancy from what you wrote to my wife that there has been some report of my doings about the School Board in Germany. So I send you the number of the *Contemporary Review* † for

* Aged 13 and 12 respectively.

† Containing his article on “The School Boards,” etc.

All our children but Jessie have the whooping-cough—*Per-tussis*—I don't know your German name for it.—It is distressing enough for them, but, I think, still worse for their mother. However, there are no serious symptoms, and I hope the change of air will set them right.

They all join with me in best wishes and regrets that you are not coming. Won't you change your mind? We start on July 31st.—Ever yours faithfully,

T. H. HUXLEY.

The summer holiday of 1871 was spent at St. Andrews, a place rather laborious of approach at that time, with all the impedimenta of a large and young family, but chosen on account of its nearness to Edinburgh, where the British Association met that year. I well remember the night journey of some ten or eleven hours, the freshness of the early morning at Edinburgh, the hasty excursion with my father up the hill from the station as far as the old High Street. The return journey, however, was made easier by the kindness of Dr. Matthews Duncan, who put up the whole family for a night, so as to break the journey.

We stayed at Castlemount, now belonging to Miss Paton, just opposite the ruined castle. Among other visitors to St. Andrews known to my father were Professors Tait and Crum Brown, who inveigled him into making trial of the "Royal and Ancient" game, which then, as now, was the staple resource of the famous little city. I have a vivid recollection of his being hopelessly bunkered three or four holes from home, and can testify that he bore the moral strain with more than usual calm as compared with the generality of golfers. Indeed, despite his naturally quick temper and his four years of naval service at a time when, perhaps, the traditions of a former generation had not wholly died out, he had a special aversion to the use of expletives; and the occasional appearance of a strong word in his letters must be put down to a simple literary use which he would have studiously avoided in conversation. A curious physical result followed the vigour with which he threw himself into the unwonted recreation. For the last twenty years his only physical exercise had been walk-

science. For Mr. Mivart, while twitting the generality of men of science with their ignorance of the real doctrines of his church, gave a reference to the Jesuit theologian Suarez, the latest great representative of scholasticism, as following St. Augustine in asserting, not direct, but derivative creation, that is to say, evolution from primordial matter endued with certain powers. Startled by this statement, Huxley investigated the works of the learned Jesuit, and found not only that Mr. Mivart's reference to the *Metaphysical Disputations* was not to the point, but that in the "*Tractatus de opere sex Dierum*," Suarez expressly and emphatically rejects this doctrine and reprehends Augustine for asserting it.

By great good luck (he writes to Darwin from St. Andrews) there is an excellent library here, with a good copy of Suarez, in a dozen big folios. Among these I dived, to the great astonishment of the librarian, and looking into them as "the careful robin eyes the delver's toil" (*vide Idylls*), I carried off the two venerable clasped volumes which were most promising.

So I have come out in the new character of a defender of Catholic orthodoxy, and upset Mivart out of the mouth of his own prophet.

Darwin himself was more than pleased with the article, and wrote enthusiastically (see *Life and Letters*, iii. 148–150). A few of his generous words may be quoted to show the rate at which he valued his friend's championship.

What a wonderful man you are to grapple with those old metaphysico-divinity books. . . . The pendulum is now swinging against our side, but I feel positive it will soon swing the other way; and no mortal man will do half as much as you in giving it a start in the right direction, as you did at the first commencement.

And again, after "mounting climax on climax," he continues:—"I must tell you what Hooker said to me a few years ago. 'When I read Huxley, I feel quite infantile in intellect.'"

This sketch of what constituted his holiday—and it was not very much busier than many another holiday—may possibly suggest what his busy time must have been like.

Till the end of the year the immense amount of work did not apparently tell upon him. He rejoiced in it. In December he remarked to his wife that with all his different irons in the fire, he had never felt his mind clearer or his vigour greater. Within a week he broke down quite suddenly, and could neither work nor think. He refers to this in the following letter:—

JERMYN STREET, *Dec. 22, 1871.*

MY DEAR JOHNNY—You are certainly improving. As a practitioner in the use of cold steel myself, I have read your letter in to-day's *Nature*, "mit Ehrfurcht und Bewunderung." And the best evidence of the greatness of your achievement is that it extracts this expression of admiration from a poor devil whose brains and body are in a colloid state, and who is off to Brighton for a day or two this afternoon.

God be with thee, my son, and strengthen the contents of thy gall-bladder!—Ever thine,
T. H. HUXLEY.

P.S.—Seriously, I am glad that at last a protest has been raised against the process of anonymous self-praise to which our friend is given. I spoke to Smith the other day about that dose of it in the "*Quarterly*" article on Spirit-rapping.

CHAPTER XXVI

1872

DYSPEPSIA, that most distressing of maladies, had laid firm hold upon him. He was compelled to take entire rest for a time. But his first holiday produced no lasting effect, and in the summer he was again very ill. Then the worry of a troublesome lawsuit in connection with the building of his new house intensified both bodily illness and mental depression. He had great fears of being saddled with heavy costs at the moment when he was least capable of meeting any new expense—hardly able even to afford another much-needed spell of rest. But in his case, as in others, at this critical moment the circle of fellow-workers in science to whom he was bound by ties of friendship, resolved that he should at least not lack the means of recovery. In their name Charles Darwin wrote him the following letter, of which it is difficult to say whether it does more honour to him who sent it or to him who received it:—

DOWN, BECKENHAM, KENT, *April 23, 1873.*

MY DEAR HUXLEY—I have been asked by some of your friends (eighteen in number) to inform you that they have placed through Robarts, Lubbock & Company, the sum of £2100 to your account at your bankers. We have done this to enable you to get such complete rest as you may require for the re-establishment of your health; and in doing this we are convinced that we act for the public interest, as well as in accordance with our most earnest desires. Let me assure you that we are all your warm personal friends, and that there is not a stranger or mere acquaintance amongst us. If you could have heard what was said, or could have read what was, as I believe, our inmost thoughts, you would know that we all feel towards

last year. Doctors talk seriously to me, and declare that all sorts of wonderful things will happen if I do not take some more efficient rest than I have had for a long time. My wife adds her quota of persuasion and admonition, until I really begin to think I must do something, if only to have peace.

What if I were to come and look you up in Naples, somewhere in February, as soon as my lectures are over?

The "one-plate system" might cure me of my incessant dyspeptic nausea. A detestable grub—larva of *Ephestia elatella*—has been devouring Her Majesty's stores of biscuits at Gibraltar. I have had to look into his origin, history, and best way of circumventing him—and maybe I shall visit Gibraltar and perhaps Malta. In that case, you will see me turn up some of these days at the Palazzo Torlonia.

Herbert Spencer has written a friendly attack on "Administrative Nihilism," which I will send you; in the same number of the *Fortnightly* there is an absurd epicene splutter on the same subject by Mill's step-daughter, Miss Helen Taylor. I intended to publish the paper separately, with a note about Spencer's criticism, but I have had no energy nor faculty to do anything lately.

Tell Lankester, with best regards, that I believe the teaching of teachers in 1872 is arranged, and that I shall look for his help in due course.

The "Happy family" have had the measles since you saw them, but they are well again.

I write in Jermyn Street, so they cannot send messages; otherwise there would be a chorus from them and the wife of good wishes and kind remembrances.—Ever yours,

T. H. HUXLEY.

He left Southampton on January 11, in the *Malta*. On the 16th, he notes in his diary, "I was up just in time to see the great portal of the Mediterranean well. It was a lovely morning, and nothing could be grander than Ape Hill on one side and the Rock on the other, looking like great lions or sphinxes on each side of a gateway."

The morning after his arrival he breakfasted with Admiral Hornby, who sent him over to Tangier in the *Helicon*, giving the Bishop of Gibraltar a passage at the same time. This led him to note down, "How the naval men love Baxter and all his works." A letter from Dr. Hooker to

his good pleasure for the last scraps ; of the natives working at the endless shadoofs ; of a group of listeners around a professional story-teller—unfinished, for he was observed sketching them.

Egypt left a profound impression upon him. His artistic delight in it apart, the antiquities and geology of the country were a vivid illustration to his trained eye of the history of man and the influence upon him of the surrounding country, the link between geography and history.

He left behind him for a while a most unexpected memorial of his visit. A friend not long after going to the pyramids, was delighted to find himself thus adjured by a donkey-boy, who tried to cut out his rival with “ Not him donkey, sah ; him donkey bad, sah ; my donkey good ; my donkey 'Fessor-uxley donkey, sah.” It appears that the Cairo donkey-boys have a way of naming their animals after celebrities whom they have borne on their backs.

While at Thebes, on his way down the river again, he received news of the death of the second son of Matthew Arnold, to whom he wrote the following letter :—

THEBES, *March* 10, 1872.

MY DEAR ARNOLD—I cannot tell you how shocked I was to see in the papers we received yesterday the announcement of the terrible blow which has fallen upon Mrs. Arnold and yourself.

Your poor boy looked such a fine manly fellow the last time I saw him, when we dined at your house, that I had to read the paragraph over and over again before I could bring myself to believe what I read. And it is such a grievous opening of a wound hardly yet healed that I hardly dare to think of the grief which must have bowed down Mrs. Arnold and yourself.

I hardly know whether I do well in writing to you. If such trouble befel me there are very few people in the world from whom I could bear even sympathy—but you would be one of them, and therefore I hope that you will forgive a condolence which will reach you so late as to disturb rather than soothe, for the sake of the hearty affection which dictates it.

My wife has told me of the very kind letter you wrote her. I was thoroughly broken down when I left England, and did not get much better until I fell into the utter and absolute

enabled me to fulfil all the engagements I had made before starting; and Donnelly had given me to understand that "My Lords" would not trouble their heads about my stretching my official leave. Nevertheless I was very glad to find the official extension (which was the effect of my wife's and your and Bence Jones's friendly conspiracy) awaiting me at Cairo. A rapid journey home *via* Brindisi might have rattled my brains back into the colloid state in which they were when I left England. Looking back through the past six months I begin to see that I have had a narrow escape from a bad break-down, and I am full of good resolutions.

As the first-fruit of these you see that I have given up the school-board, and I mean to keep clear of all that semi-political work hereafter. I see that Sandon (whom I met at Alexandria) and Miller have followed my example, and that Lord Lawrence is likely to go. What a skedaddle!

It seems very hard to escape, however. Since my arrival here, on taking up the *Times* I saw a paragraph about the Lord Rectorship of St. Andrews. After enumerating a lot of candidates for that honour, the paragraph concluded, "But we understand that at present Professor Huxley has the best chance." It is really too bad if anyone has been making use of my name without my permission. But I don't know what to do about it. I had half a mind to write to Tulloch to tell him that I can't and won't take any such office, but I should look rather foolish if he replied that it was a mere newspaper report, and that nobody intended to put me up.

Egypt interested me profoundly, but I must reserve the tale of all I did and saw there for word of mouth. From Alexandria I went to Messina, and thence made an excursion along the lovely Sicilian coast to Catania and Etna. The old giant was half covered with snow, and this fact, which would have tempted you to go to the top, stopped me. But I went to the Val del Bove, whence all the great lava streams have flowed for the last two centuries, and feasted my eyes with its rugged grandeur. From Messina I came on here, and had the great good fortune to find Vesuvius in eruption. Before this fact the vision of good Bence Jones forbidding much exertion vanished into thin air, and on Thursday up I went in company with Ray Lankester and my friend Dohrn's father, Dohrn himself being unluckily away. We had a glorious day, and did not descend till late at night. The great crater was not very active, and contented itself with throwing out great clouds of steam and volleys of

as soon as he began to work again in London, his old enemy returned. Early hours, the avoidance of society and societies, an hour's riding before starting at nine for South Kensington, were all useless; the whole year was poisoned until a special diet prescribed by Dr. (afterwards Sir) Andrew Clark, followed by another trip abroad, effected a cure. I remember his saying once that he learned by sad experience that such a holiday as that in Egypt was no good for him. What he really required was mountain air and plenty of exercise. The following letters fill up the outline of this period:—

26 ABBEY PLACE, *May* 20, 1872.

MY DEAR DOHRN — I suppose that you are now back in Naples, perambulating the Chiaja, and looking ruefully on the accumulation of ashes on the foundations of the aquarium! The papers, at any rate, tell us that the ashes of Vesuvius have fallen abundantly at Naples. Moreover, that abominable municipality is sure to have made the eruption an excuse for all sorts of delays. May the gods give you an extra share of temper and patience!

What an unlucky dog our poor Ray is, to go and get fever when of all times in the world's history he should not have had it. However, I hear he is better and on his way home. I hope he will be well enough when he returns not only to get his Fellowship, but to help me in my schoolmaster work in June and July.

I was greatly disgusted to miss you in Naples, but it was something to find your father instead. What a vigorous, genial *youngster* of three score and ten he is. I declare I felt quite aged beside him. We had a glorious day on Vesuvius, and behaved very badly by leaving him at the inn for I do not know how many hours, while we wandered about the cone. But he had a very charming young lady for companion, and possibly had the best of it. I am very sorry that at the last I went off in a hurry without saying "Good-bye" to him, but I desired Lankester to explain, and I am sure he will have sympathised with my anxiety to see Rome.

I returned, thinking myself very well, but a bad fit of dyspepsia seized me, and I found myself obliged to be very idle and very careful of myself—neither of which things are to my taste. But I am right again now, and hope to have no more backslidings. However, I am afraid I may not be able to attend

I have no doubt you will get a letter from him in three weeks or so. His name is Gregory, and you will find him a good-humoured acute man of the world, with a very great general interest in scientific and artistic matters. Indeed in art I believe he is a considerable connoisseur.

I am very grieved to hear of your father's serious illness. At his age cerebral attacks are serious, and when we spent so many pleasant hours together at Naples, he seemed to have an endless store of vigour—very much like his son Anton.

What put it into your head that I had any doubt of your power of work? I am ready to believe that you are Hydra in the matter of heads and Briareus in the matter of hands.

. . . If you go to Ceylon I shall expect you to come back by way of England. It's the shortest route anywhere from India, though it may not look so on the map.

How am I? Oh, getting along and just keeping the devil of dyspepsia at arm's length. The wife and other members of the H. F. are well, and would send you greetings if they knew I was writing to you.—Ever yours faithfully,

T. H. HUXLEY.

A little later Von Willemoes Suhm ("why the deuce does he have such a long name, instead of a handy monosyllable or dissyllable like Dohrn or Huxley?") was recommended for the post. He afterwards was one of the scientific staff of the *Challenger*, and died during the voyage.

MORTHOE, NEAR BARNSTAPLE, NORTH DEVON,
Aug. 5, 1872.

MY DEAR DOHRN—I trust you have not been very wroth with me for my long delay in answering your last letter. For the last six weeks I have been very busy lecturing daily to a batch of schoolmasters, and looking after their practical instruction in the laboratory which the Government has, at last, given me. In the "intervals of business" I have been taking my share in a battle which has been raging between my friend Hooker of Kew and his official chief. . . . And moreover I have just had strength enough to get my daily work done and no more, and everything that could be put off has gone to the wall. Three days ago, the "Happy Family," bag and baggage, came to this remote corner, where I propose to take a couple of months' entire rest—and put myself in order for next winter's campaign. It is a little village five miles from the nearest town

content myself, for many years, with what seemed the next best thing, namely, as full an exposition as I could give, of the characters of certain plants and animals, selected as types of vegetable and animal organisation, by way of introduction to systematic zoology and paleontology.

There was no laboratory work, but he would show an experiment or a dissection during the lecture or perhaps for a few minutes after, when the audience crowded round the lecture table.

The opportunity came in 1871. As he afterwards impressed upon the great city companies in regard to technical education, the teaching of science throughout the country turned upon the supply of trained teachers. The part to be played by elementary science under the Education Act of 1870, added urgency to the question of proper teaching. With this in view, he organised a course of instruction for those who had been preparing pupils for the examinations of the Science and Art Department, "scientific missionaries," as he described them to Dr. Dohrn.

In the promotion of the practical teaching of biology (writes the late Jeffery Parker, *Nat. Sci.* viii. 49), Huxley's services can hardly be overestimated. Botanists had always been in the habit of distributing flowers to their students, which they could dissect or not as they chose; animal histology was taught in many colleges under the name of practical physiology; and at Oxford an excellent system of zoological work had been established by the late Professor Rolleston.* But the biological laboratory, as it is now understood, may be said to date from about 1870, when Huxley, with the co-operation of Professors

* "Rolleston (Professor Lankester writes to me) was the first to systematically conduct the study of Zoology and Comparative Anatomy in this country by making use of a carefully selected series of animals. His 'types' were the Rat, the Common Pigeon, the Frog, the Perch, the Crayfish, Blackbeetle, Anodon, Snail, Earthworm, Leech, Tapeworm. He had a series of dissections of these mounted, also loose dissections and elaborate MS. descriptions. The student went through this series, dissecting fresh specimens for himself. After some ten years' experience Rolleston printed his MS. directions and notes as a book, called *Forms of Animal Life*.

"This all preceded the practical class at South Kensington in 1871. I have no doubt that Rolleston was influenced in his plan by your

familiar type of the rabbit or frog. This was Rolleston's practice; but it may be noted that Professor Ray Lankester has always maintained and further developed the "original Huxleian plan of beginning with the same microscopic forms" as being a most important philosophic improvement on Rolleston's plan, and giving, he considers, "the truer 'twist,' as it were, to a student's mind."

When the book was sent to Darwin, he wrote back (November 12, 1875):—

MY DEAR HUXLEY—Many thanks for your biology, which I have read. It was a real stroke of genius to think of such a plan. Lord, how I wish that I had gone through such a course. —Ever yours,
C. DARWIN.

A large portion of his time and energy was occupied in the organisation of this course of teaching for teachers, and its elaboration before being launched on a larger scale in October, when the Biological Department of the Jermyn Street school was transferred to the new buildings at South Kensington, fitted with laboratories which were to excite his friend Dr. Dohrn's envy. But he was also at work upon his share of the *Science Primers*, so far as his still uncertain health allowed. This and the affairs of the British Association are the subject of several letters to Sir Henry Roscoe and Dr. Tyndall.

26 ABBEY PLACE, *April* 8, 1872.

MY DEAR ROSCOE—Many thanks for your kind letter of welcome. My long rest has completely restored me. As my doctor told me, I was sound, wind and limb, and had merely worn myself out. I am not going to do that again, and you see that I have got rid of the School Board. It was an awful incubus!

Oddly enough I met the Ashtons in the Vatican, and heard about your perplexities touching Oxford. I should have advised you to do as you have done. I think that you have a great piece of work to do at Owens College, and that you will do it. If you had gone to Oxford you would have sacrificed all the momentum you have gained in Manchester; and would have had to begin *de novo*, among conditions which, I imagine, it is very hard for a non-University man to appreciate and adjust himself to.

I like the look of the "Primers" (of which Macmillan has

to become of the association if — is to monopolise it? And then there was that scoundrel, Louis Napoleon—to whom no honest man ought to speak—gracing the scene. I am right glad I was out of it.

I am at my wits' end to suggest a lecturer for you. I wish I could offer myself, but I have refused everything of that sort on the score of health; and moreover, I am afraid of my wife!

What do you say to Ramsay? He lectures very well. I have done nothing whatever to the Primer. Stewart sent me Geikie's letter this morning, and I have asked Macmillan to send Geikie the proofs of my Primer so far as they go. We must not overlap more than can be helped.

I have not seen Hooker yet since my return. While all this row has been going on, I could not ask him to do anything for us. And until X. is dead and d—d (officially at any rate), I am afraid there will be little peace for him.—Ever yours very faithfully,

T. H. HUXLEY.

Please remember me very kindly to Mrs. Roscoe.

In a letter of September 25 is a reference to the way in which his increasing family had outgrown his house in Abbey Place. Early in the preceding year, he had come to the decision to buy a small house in the same neighbourhood, and add to it so as to give elbow-room to each and all of the family. This was against the advice of his friend and legal adviser, to whom he wrote announcing his decision, as follows. The letter was adorned with a sketch of an absurd cottage, "Ye House!" perched like a windmill on a kind of pedestal, and with members of the family painfully ascending a ladder to the upper storey, above the ominous legend, "Staircase forgotten."

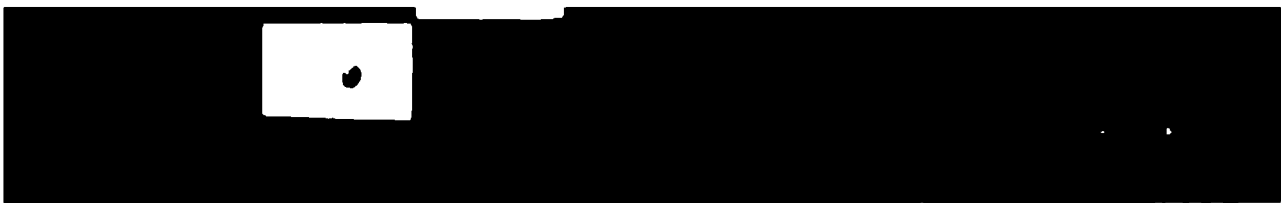
March 20, 1871.

MY DEAR BURTON—There is something delightfully refreshing in rushing into a piece of practical work in the teeth of one's legal adviser.

If the lease of a piece of ground whereon I am going to build mine house come to you, will you see if it's all right.—Yours wilfully,

T. H. HUXLEY.

This house, No. 4 Marlborough Place, stands on the north side of that quiet street, close to its junction with Abbey Road. It is next door to the Presbyterian Church,



Happily the man turned out to have enough means to pay the bulk of the costs; but that was no compensation for the mental worry and consequent ill-health entailed from November to June.

The only amusing point in the whole affair was when the plaintiff's solicitors had the face to file an affidavit before the Vice-Chancellor himself in answer to his strictures upon the case, "about as regular a proceeding," reports Mr. Burton, "as for a middy to reply upon the Post Captain on his own quarter-deck."

The move was made in the third week of December (1872) amid endless rain and mud and with workmen still in the house. It was attended by one inconvenience. He writes to Darwin on December 20, 1872:—

I am utterly disgusted at having only just received your note of Tuesday. But the fact is, there is a certain inconvenience about having *four* addresses as has been my case for the most part of this week, in consequence of our moving—and as I have not been to Jermyn Street before to-day, I have missed your note. I should run round to Queen Anne St. now on the chance of catching you, but I am bound here by an appointment.

One incident of the move, however, was more agreeable. Mr. Herbert Spencer took the opportunity of sending a New Year's gift for the new house, in the shape of a handsome clock, wishing, as he said, "to express in some way more emphatic than by words, my sense of the many kindnesses I have received at your hands during the twenty years of our friendship. Remembrance of the things you have done in furtherance of my aims, and of the invaluable critical aid you have given me, with so much patience and at so much cost of time, has often made me feel how much I owe you."

After a generous reference to occasions when the warmth of debate might have betrayed him into more vigorous expressions than he intended, he concludes:—

But inadequately as I may ordinarily show it, you will (knowing that I am tolerably candid) believe me when I say that there is no one whose judgment on all subjects I so much respect, or whose friendship I so highly value.

days' work—if the fact that Mr. H—— has already filled the office of anatomical Demonstrator (as I understand from you) does not satisfy them as to his competency.—I am, dear Madam,
yours very faithfully, T. H. HUXLEY

Miss S. JEX BLAKE.

The last event of the year was that he was elected by the students Lord Rector of Aberdeen University—a position, the duties of which consist partly in attending certain meetings of the University Court, but more especially in delivering an address. This, however, was not required for another twelvemonth, and the address on “Universities, Actual and Ideal,” was delivered in fulfilment of this duty on February 1874.

CHAPTER XXVII

1873

THE year opens with a letter to Tyndall, then on a lecturing tour in America:—

4 MARLBOROUGH PLACE, ABBEY ROAD, N.W.,
January 1, 1872 [1873].

MY DEAR TYNDALL—I cannot let this day go by without wishing you a happy New Year, and lamenting your absence from our customary dinner. But Hirst and Spencer and Michael Foster are coming, and they shall drink your health in champagne while I do the like in cold water, making up by the strength of my good wishes for the weakness of the beverage.

You see I write from the new house. Getting into it was an awful job, made worse than needful by the infamous weather we have had for weeks and months, and by the stupid delays of the workmen whom we had fairly to shove out at last as we came in. We are settling down by degrees, and shall be very comfortable by and by, though I do not suppose that we shall be able to use the drawing-room for two or three months to come. I am very glad to have made the change, but there is a drawback to everything in “this here wale,” as Mrs. Gamp says, and my present thorn in the flesh is a neighbour, who says I have injured him by certain operations in my garden, and is trying to get something out of me by Chancery proceedings. Fancy finding myself a defendant in Chancery!

It is particularly hard on me, as I have been especially careful to have nothing done without Burton’s sanction and assurance that I was quite safe in law; and I would have given up anything rather than have got into bother of this kind. But “sich is life.”

You seem to have been making a Royal Progress in Yankee-land. We have been uncommonly tickled with some of the

reports of your lectures which reached us, especially with that which spoke of your having "a strong English accent."

The loss of your assistant seems to have been the only deduction to be made from your success. I am afraid you must have felt it much in all ways.

"My Lord" received your telegram only after the business of "securing Hirst" was done. That is one of the bright spots in a bad year for me. Goschen consulted Spottiswoode and me independently about the headship of the new Naval College, and was naturally considerably surprised by the fact that we coincided in recommending Hirst. . . . The upshot was that Goschen asked me to communicate with Hirst and see if he would be disposed to accept the offer. So I did, and found to my great satisfaction that Hirst took to the notion very kindly. I am sure he is the very best man for the post to be met with in the three kingdoms, having that rare combination of qualities by which he gets on with all manner of men, and singularly attracts young fellows. He will not only do his duty, but be beloved for doing it, which is what few people can compass.

I have little news to give you. The tail of the X.-Hooker storm is drifting over the scientific sky in the shape of fresh attacks by Owen on Hooker. Hooker answered the last angelically, and I hope they are at an end.

The wife has just come in and sends her love (but is careful to add "second-best"). The chicks grow visibly and audibly, and Jess looks quite a woman. All are well except myself, and I am getting better from a fresh breakdown of dyspepsia. I find that if I am to exist at all it must be on strictly ascetic principles, so there is hope of my dying in the odour of sanctity yet. If you recollect, Lancelot did not know that he should "die a holy man" till rather late in life. I have forgotten to tell you about the Rectorship of Aberdeen. I refused to stand at first, on the score of health, and only consented on condition that I should not be called upon to do any public work until after the long vacation. It was a very hard fight, and although I had an absolute majority of over fifty, the mode of election is such that one vote, in one of the four nations, would have turned the scale by giving my opponent the majority in that nation. We should then have been ties, and as the chancellor, who has under such circumstances a casting vote, would have (I believe) given it against me, I should have been beaten.

As it is, the fact of anyone, who stinketh in the nostrils of orthodoxy, beating a Scotch peer at his own gates in the most

orthodox of Scotch cities, is a curious sign of the times. The reason why they made such a tremendous fight for me, is I believe, that I may carry on the reforms commenced by Grant Duff, my predecessor. Unlike other Lord Rectors, he of Aberdeen is a power and can practically govern the action of the University during his tenure of office.

I saw Pollock yesterday, and he says that they want you back again. Curiously the same desire is epidemically prevalent among your friends, not least here.—Ever yours,

T. H. HUXLEY.

In spite of his anxieties, his health was slowly improving under careful regimen. He published no scientific memoirs this year, but in addition to his regular lectures, he was working to finish his *Manual of Invertebrate Anatomy* and his *Introductory Primer*, and to write his Aberdeen address; he was also at work upon the *Pedigree of the Horse* and on *Bodily Motion and Consciousness*. He delivered a course to teachers on Psychology and Physiology, and was much occupied by the Royal Commission on Science. As a governor of Owens College he had various meetings to attend, though his duties did not extend, as some of his friends seem to have thought, to the appointment of a Professor of Physiology there.

My life (he writes to Sir Henry Roscoe) is becoming a burden to me because of ——. Why I do not know, but for some reason people have taken it into their heads that I have something to do with appointments in Owens College, and no fewer than three men of whose opinion I think highly have spoken or written to me urging ——'s merits very strongly.

This summer he again took a long holiday, thanks to the generosity of his friends (see p. 394), and with better results. He went with his old friend Hooker to the Auvergne, walking, geologising, sketching and gradually discarding doctor's orders. Sir Joseph Hooker has very kindly written me a letter from which I give an account of this trip:—

It was during the many excursions we took together, either by ourselves or with one of my boys, that I knew him best at his best; and especially during one of several weeks' duration in

the summer of 1873, which we spent in central France and Germany. He had been seriously ill, and was suffering from severe mental depression. For this he was ordered abroad by his physician, Sir A. Clark, to which step he offered a stubborn resistance. With Mrs. Huxley's approval, and being myself quite in the mood for a holiday, I volunteered to wrestle with him, and succeeded, holding out as an inducement a visit to the volcanic region of the Auvergne with Scrope's classical volume, which we both knew and admired, as a guide book.

We started on July 2nd, I loaded with injunctions from his physician as to what his patient was to eat, drink, and avoid, how much he was to sleep and rest, how little to talk and walk, etc., that would have made the expedition a perpetual burthen to me had I not believed that I knew enough of my friend's disposition and ailments to be convinced that not only health but happiness would be our companions throughout. Sure enough, for the first few days, including a short stay in Paris, his spirits were low indeed, but this gave me the opportunity of appreciating his remarkable command over himself and his ever present consideration for his companion. Not a word or gesture of irritation ever escaped him; he exerted himself to obey the instructions laid down; nay, more, he was instant in his endeavour to save me trouble at hotels, railway stations, and ticket offices. Still, some mental recreation was required to expedite recovery, and he found it first by picking up at a bookstall, a *History of the Miracles of Lourdes*, which were then exciting the religious fervour of France, and the interest of her scientific public. He entered with enthusiasm into the subject, getting together all the treatises upon it, favourable or the reverse, that were accessible, and I need hardly add, soon arrived at the conclusion, that the so-called miracles were in part illusions and for the rest delusions. As it may interest some of your readers to know what his opinion was in this the early stage of the manifestations, I will give it as he gave it to me. It was a case of two peasant children sent in the hottest month of the year into a hot valley to collect sticks for firewood washed up by a stream, when one of them after stooping down opposite a heat-roasting rock, was, in rising, attacked with a transient vertigo under which she saw a figure in white against the rock. The bare fact being reported to the cure of the village, all the rest followed.

Soon after our arrival at Clermont-Ferrand your father had so far recovered his wonted elasticity of spirit that he took a keen interest in everything around the museum, the cathedral,

where he enjoyed the conclusion of the service by a military band which gave selections from the *Figlia del Regimento*, but above all he appreciated the walks and drives to the geological features of the environs. He reluctantly refrained from ascending the Puy de Dome, but managed the Pic Parion, Gergovia, Royat, and other points of interest without fatigue. . . .

After Clermont they visited the other four great volcanic areas explored by Scrope, Mont Dore, the Cantal, Le Puy, and the valley of the Ardèche. Under the care of his friend, and relieved from the strain of work, my father's health rapidly improved. He felt no bad effects from a night at Mont Dore, when, owing to the crowd of invalids in the little town, no better accommodation could be found than a couple of planks in a cupboard. Next day they took up their quarters in an unpretentious cabaret at La Tour d'Auvergne, one of the villages on the slopes of the mountain, a few miles away.

Here (writes Sir J. Hooker), and for some time afterwards, on our further travels, we had many interesting and amusing experiences of rural life in the wilder parts of central France, its poverty, penury, and too often its inconceivable impositions and overcharges to foreigners, quite consistently with good feeling, politeness, and readiness to assist in many ways.

By the 10th of July, nine days after setting out, I felt satisfied (he continues) that your father was equal to an excursion upon which he had set his heart, to the top of the Pic de Sancy, 4000 feet above La Tour and 7 miles distant.

It was on this occasion that the friends made what they thought a new discovery, namely evidence of glacial action in central France. Besides striated stones in the fields or built into the walls, they noticed the glaciated appearance of one of the valleys descending from the peak, and especially some isolated gigantic masses of rock on an open part of the valley, several miles away, as to which they debated whether they were low buildings or transported blocks. Sir Joseph visited them next day, and found they were the latter, brought down from the upper part of the peak.*

* He published an account of these blocks in *Nature*, xiii. 31, 166, but subsequently found that glaciation had been observed by von Lassaul in 1872 and by Sir William Guise in 1870.

The following letters to Sir H. Roscoe and Dr. Tyndall were written during this tour:—

LE PUY, HAUTE LOIRE, FRANCE,
July 17, 1873.

MY DEAR ROSCOE—Your very kind letter reached me just as I was in the hurry of getting away from England, and I have been carrying it about in my pocket ever since.

Hooker and I have been having a charming time of it among the volcanoes of the Auvergne, and we are now on our way to those of the Velay and Vivarrais. The weather has been almost perfect. Perhaps a few degrees of temperature could have been spared now and then, especially at Clermont, of which somebody once said that having stayed there the climate of hell would have no terrors for him.

It has been warm in the Mont Dore country and in the Cantal, as it is here, but we are very high up, and there is a charming freshness and purity about the air.

I do not expect to be back before the end of September, and my lectures begin somewhere in the second week of October. After they commence I shall not be able to leave London even for a day, but I shall be very glad to come to the inauguration of your new buildings if the ceremony falls within my possible time. And you know I am always glad to be your guest.

I am thriving wonderfully. Indeed all that plagues me now is my conscience, for idling about when I feel full of vigour. But I promised to be obedient, and I am behaving better than Auld Cloutie did when he fell sick.

I hope you are routing out the gout. This would be the place for you—any quantity of mineral waters.

Pray remember me very kindly to Mrs. Roscoe, and believe me, ever yours very faithfully,

T. H. HUXLEY.

HOTEL DE FRANCE, BADEN-BADEN,
July 30, 1873.

MY DEAR TYNDALL—We find ourselves here after a very successful cruise in the Auvergne and Ardèche, successful at least so far as beauty and geological interest go. The heat was killing, and obliged us to give up all notion of going to Ursines, as we had at first intended to do. So we turned our faces north and made for Grenoble, hoping for a breath of cool air from the mountains of Dauphiny. But Grenoble was hotter even than

and really only the continuation of that movement. But there is nothing new in the ideas which lie at the bottom of the movement, nor is any reconciliation possible between free thought and traditional authority. One or other will have to succumb after a struggle of unknown duration, which will have as side issues vast political and social troubles. I have no more doubt that free thought will win in the long run than I have that I sit here writing to you, or that this free thought will organise itself into a coherent system, embracing human life and the world as one harmonious whole. But this organisation will be the work of generations of men, and those who further it most will be those who teach men to rest in no lie, and to rest in no verbal delusions. I may be able to help a little in this direction—perhaps I may have helped already. For the present, however, I am disposed to draw myself back entirely into my own branch of physical science. There is enough and to spare for me to do in that line, and, for years to come, I do not mean to be tempted out of it.

Strangely enough, this was the one thing he was destined not to do. Official work multiplied about him. From 1870 to 1884 only two years passed without his serving on one or two Royal Commissions. He was Secretary of the Royal Society from 1871 to 1880, and President from 1883 to his retirement, owing to ill-health, in 1885. He became Dean as well as Professor of Biology in the College of Science, and Inspector of Fisheries. Though he still managed to find some time for anatomical investigations, and would steal a precious hour or half hour by driving back from the Home Office to his laboratory at South Kensington before returning home to St. John's Wood, the amount of such work as he was able to publish could not be very great.

His most important contributions during this decennium (writes Sir M. Foster) were in part continuations of his former labours, such as the paper and subsequent full memoir on *Stagonolepis*, which appeared in 1875 and 1877, and papers on the Skull. The facts that he called a communication to the Royal Society, in 1875,* on *Amphioxus*, a preliminary note, and that a paper read to the Zoological Society in 1876, on

* Written 1874.

him which made his hand, when turned to anatomical science, so heavy that he could not lift it. Not even that which was so strong within him, the duty of fulfilling a promise, could bring him to the work. In his room at South Kensington, where for a quarter of a century he had laboured with such brilliant effect, there lay on his working table for months, indeed for years, partly dissected specimens of the rare and little studied marine animal, *Spirula*, of which he had promised to contribute an account to the Reports of the "Challenger" Expedition, and hard by lay the already engraven plates; there was still wanted nothing more than some further investigation and the working out of the results. But it seemed as if some hidden hands were always being stretched out to keep him from the task; and eventually another labourer had to complete it. (*Ibid.*)

The remaining letters of this year include several to Dr. Dohrn, which show the continued interest my father took in the great project of the Biological Station at Naples, which was carried through in spite of many difficulties. He had various books and proceedings of learned societies sent out at Dr. Dohrn's request (I omit the details) and proposed a scheme for raising funds towards completing the building when the contractor failed. The scheme, however, was not put into execution.

4 MARLBOROUGH PLACE, *Feb.* 24, 1873.

MY DEAR DOHRN—I was very glad to receive the fine sealed letter, and to get some news of you—though to be sure there is not much of you in the letter, but all is "Station, Station."

I congratulate you heartily on your success with your undertaking, and I only wish I could see England represented among the applicants for tables. But you see England is so poor, and the present price of coals obliges her to economise.

I envy you your visit from "Pater Anchises" Baer, and rejoice to hear that the grand old man is well and strong enough to entertain such a project. I wish I could see my way to doing the like. I have had a long bout of illness—ever since August—but I am now very much better, indeed, I hope I may say quite well. The weariness of all this has been complicated by the trouble of getting into a new house, and in addition a law-suit brought by a knavish neighbour, in the hope of extracting money out of me.

I am happy to say, however, that he has just been thoroughly and effectually defeated. It has been a new experience for me,

There is a rub for you. If you write to me in English again I will send the letter back without paying the postage.

In any case let me have a precise statement of your financial position. I may have a chance of talking to some Croesus, and the first question he is sure to ask me is—How am I to know that this is a stable affair, and that I am not throwing my money into the sea? . . .

(Referring to an unpleasant step it seemed necessary to take) . . . you must make up your mind to act decidedly and take the consequences. No good is ever done in this world by hesitation. . . .

I hope you are physically better. Look sharply after your diet, take exercise and defy the blue-devils, and you will weather the storm.—Ever yours very faithfully, T. H. HUXLEY.

Tyndall, who had not attended the 1873 meeting of the British Association, had heard that some local opposition had been offered to his election as President for the Belfast meeting in 1874, and had written:—

I wish to heaven you had not persuaded me to accept that Belfast duty. They do not want me. . . . But Spottiswoode assures me that no individual offered the slightest support to the two unscientific persons who showed opposition.

The following was written in reply:—

4 MARLBOROUGH PLACE, *Sept.* 25, 1873.

MY DEAR TYNDALL—I am sure you are mistaken about the Belfast people. That blundering idiot of ——— wanted to make himself important and get up a sort of “Home Rule” agitation in the Association, but nobody backed him and he collapsed. I am at your disposition for whatever you want me to do, as you know, and I am sure Hooker is of the same mind. We shall not be ashamed when we meet our enemies in the gate.

The grace of God cannot entirely have deserted you since you are aware of the temperature of that ferocious epistle. Reeks,* whom I saw yesterday, was luxuriating in it, and said (confound his impudence) that it was quite my style. I forgot to tell him, by the bye, that I had resigned in your favour ever since the famous letter to Carpenter. Well, so long as you are better after it there is no great harm done.

* The late Trenham Reeks, Registrar of the School of Mines, and Curator of the Museum of Practical Geology.

hope of some good coming out of that Ashantee row, if only in the shape of rare vocables.

My attention is quite turned away from Anthropological matters at present, but I will bear your question in mind if opportunity offers.

A letter to Professor Rolleston at Oxford gives a lively account of his own ailments, which could only have been written by one now recovering from them, while the illness of another friend raised a delicate point of honour, which he laid before the judgment of Mr. Darwin, more especially as the latter had been primarily concerned in the case.

4 MARLBOROUGH PLACE, *Oct.* 16, 1873.

MY DEAR ROLLESTON—A note which came from Mrs. Rolleston to my wife the other day, kindly answering some inquiries of ours about the Oxford Middle Class Examination, gave us but a poor account of your health.

This kind of thing won't do, you know. Here is — ill, and I doing all I can to persuade him to go away and take care of himself, and now comes ill news of you.

Is it dyspeps again? If so follow in my steps. I mean to go about the country, with somebody who can lecture, as the "horrid example"—cured. Nothing but gross and disgusting intemperance, Sir, was the cause of all my evil. And now that I have been a teetotaller for nine months, and have cut down my food supply to about half of what I used to eat, the enemy is beaten.

I have carried my own permissive bill, and no canteen (except for my friends who still sit in darkness) is allowed on the premises. And as this is the third letter I have written before breakfast (a thing I never could achieve in the days when I wallowed in the sty of Epicurus), you perceive that I am as vigorous as ever I was in my life.

Let me have news of you, and believe me—Ever yours very faithfully,
T. H. H.

ATHENÆUM CLUB, *Nov.* 3, 1873.

MY DEAR DARWIN—You will have heard (in fact I think I mentioned the matter when I paid you my pleasant visit the other day) that — is ill and obliged to go away for six months to a warm climate. It is a great grief to me, as he is a man for whom I have great esteem and affection, apart from his

high scientific merits, and his symptoms are such as cause very grave anxiety. I shall be happily disappointed if that accursed consumption has not got hold of him.

The college authorities have behaved as well as they possibly could to him, and I do not suppose that his enforced retirement for a while gives him the least pecuniary anxiety as his people are all well off, and he himself has an income apart from his college pay. Nevertheless, under such circumstances, a man with half a dozen children always wants all the money he can lay hands on; and whether he does or no, he ought not to be allowed to deprive himself of any, which leads me to the gist of my letter. His name was on your list as one of those hearty friends who came to my rescue last year, and it was the only name which made me a little uneasy, for I doubted whether it was right for a man with his responsibilities to make sacrifices of this sort. However, I stifled that feeling, not seeing what else I could do without wounding him. But now my conscience won't let me be, and I do not think that any consideration ought to deter me from getting his contribution back to him somehow or other. There is no one to whose judgment on a point of honour I would defer more readily than yours, and I am quite sure you will agree with me. I really am quite unhappy and ashamed to think of myself as vigorous and well at the expense of his denying himself any rich man's caprice he might take a fancy to.

So, my dear, good friend, let me know what his contribution was, that I may get it back to him somehow or other, even if I go like Nicodemus privily and by night to his bankers.—Ever yours faithfully,

T. H. H.

CHAPTER XXVIII

1874

My father's health continued fairly good in 1874, and while careful to avoid excessive strain he was able to undertake nearly as much as before his illness outside his regular work at South Kensington, the Royal Society, and on the Royal Commission. To this year belong three important essays, educational and philosophical. From February 25 to March 3 he was at Aberdeen, staying first with Professor Bain, afterwards with Mr. Webster, in fulfilment of his first duty as Lord Rector * to deliver an address to the students. Taking as his subject "Universities, Actual and Ideal," he then proceeded to vindicate, historically and philosophically, the claims of natural science to take the place from which it had so long been ousted in the universal culture which a University professes to give. More especially he demanded an improved system of education in the medical school, a point to which he gave practical effect in the Council of the University.

In an ideal University, as I conceive it, a man should be able to obtain instruction in all forms of knowledge, and discipline in the use of all the methods by which knowledge is obtained. In such a University the force of living example should fire the student with a noble ambition to emulate the learning of learned men, and to follow in the footsteps of the explorers of new fields of knowledge. And the very air he breathes should be charged

* It may be noted that between 1860 and 1890 he and Professor Bain were the only Lord Rectors of Aberdeen University elected on non-political grounds.

with that enthusiasm for truth, that fanaticism of veracity, which is a greater possession than much learning; a nobler gift than the power of increasing knowledge; by so much greater and nobler than these, as the moral nature of man is greater than the intellectual; for veracity is the heart of morality. (*Coll. Ess.* iii, 189, *sqq.*)

As for the “so-called ‘conflict of studies,’” he exclaims—

One might as well inquire which of the terms of a Rule of Three sum one ought to know in order to get a trustworthy result. Practical life is such a sum, in which your duty multiplied into your capacity and divided by your circumstances gives you the fourth term in the proportion, which is your deserts, with great accuracy.

The knowledge on which medical practice should be based is “the sort of practical, familiar, finger-end knowledge which a watchmaker has of a watch,” the knowledge gained in the dissecting-room and laboratory,

Until each of the greater truths of anatomy and physiology has become an organic part of your minds—until you would know them if you were roused and questioned in the middle of the night, as a man knows the geography of his native place and the daily life of his home. That is the sort of knowledge which, once obtained, is a life-long possession. Other occupations may fill your minds—it may grow dim and seem to be forgotten—but there it is, like the inscription on a battered and defaced coin, which comes out when you warm it.

Hence the necessity to concentrate the attention on these cardinal truths, and to discard a number of extraneous subjects commonly supposed to be requisite whether for general culture of the medical student or to enable him to correct the possible mistakes of druggists. Against this “Latin fetish” in medical education, as he used to call it, he carried on a lifelong campaign, as may be gathered from his published essays on medical education, and from letters given in later chapters of this book. But there is another side to such limitation in professional training. Though literature is an essential in the preliminary, general education, culture is not solely dependent upon classics.

Moreover, I would urge that a thorough study of Human Physiology is in itself an education broader and more comprehensive than much that passes under that name. There is no side of the intellect which it does not call into play, no region of human knowledge into which either its roots or its branches do not extend; like the Atlantic between the Old and the New Worlds, its waves wash the shores of the two worlds of matter and of mind; its tributary streams flow from both; through its waters, as yet unfurrowed by the keel of any Columbus, lies the road, if such there be, from the one to the other; far away from that North-west Passage of mere speculation, in which so many brave souls have been hopelessly frozen up.

Of the address he writes to his wife, February 27:—

I have just come back from the hall in which the address was delivered, somewhat tired. The hall was very large, and contained, I suppose, a couple of thousand people, and the students made a terrific row at intervals, though they were quiet enough at times. As the address took me an hour and a half to deliver, and my voice has been very shaky ever since I have been here, I did not dare to put too much strain upon it, and I suspect that the people at the end of the hall could have heard very little. However, on the whole, it went off better than I expected.

And to Professor Baynes:—

I am very glad you liked my address. The students were abnormally quiet for the first half hour, and then made up for their reticence by a regular charivari for the rest of the time. However, I was consoled by hearing that they were much quieter than usual.

Dr. John Muir's appreciation is worth having. It did not occur to me that what I had to say would interest people out of Britain, but to my surprise I had an application from a German for permission to translate the address the other day.

Again to his wife, March 1:—

. . . I was considerably tired after my screed on Friday, but Bain and I took a long walk, and I was fresh again by dinner-time. I dined with the Senators at a hotel in the town, and of course had to make a speech or two. However I cut all that as fast as I could. They were all very apologetic for the row the students made. After the dinner one of the Professors came

asked by Professor Ray Lankester, then a Fellow of Exeter College, if he could not break his journey there, and inspect the results of his investigations on Lymnæus. The answer was as follows:—

We go to Birmingham on Friday by the three o'clock train, but there is no chance of stopping at Oxford either going or coming, so that unless you bring a Lymnæus or two (under guise of periwinkles for refreshment) to the carriage door I shall not be able to see them.

The following letters refer both to this address on Priestley, and to the third of the important addresses of this year, that "On the Hypothesis that Animals are Automata, and its History" (*Coll. Ess.* i. 199, see also p. 442 below). The latter was delivered at Belfast before the British Association under Tyndall's presidency. It appears that only a month before, he had not so much as decided upon his subject—indeed, was thinking of something quite different.

The first allusion in these letters is to a concluding phase of Tyndall's controversy upon the claims of the late Principal Forbes in the matter of Glacier theory:—

4 MARLBOROUGH PLACE, LONDON, N.W.,

June 24, 1874.

MY DEAR TYNDALL—I quite agree with your Scotch friend in his estimate of Forbes, and if he were alive and the controversy beginning I should say draw your picture in your best sepia or lamp black. But I have been thinking over this matter a good deal since I received your letter, and my verdict is, leave that tempting piece of portraiture alone.

The world is neither wise nor just, but it makes up for all its folly and injustice by being damnably sentimental, and the more severely true your portrait might be the more loud would be the outcry against it. I should say publish a new edition of your *Glaciers of the Alps*, make a clear historical statement of all the facts showing Forbes's relations to Rendu and Agassiz, and leave the matter to the judgment of your contemporaries. That will sink in and remain when all the hurly-burly is over.

I wonder if that address is begun, and if you are going to be as wise and prudent as I was at Liverpool. When I think of the temptation I resisted on that occasion, like Clive when he was charged with speculation, "I marvel at my own forbearance!"

and as vigorous as ever I was in my life. But a late dinner wakes up my demoniac colon and gives me a fit of blue devils with physical precision.

Don't believe that I am at all the places in which the newspapers put me. For example, I was not at the Lord Mayor's dinner last night. As for Lord Derby's statue, I wanted to get a lesson in the art of statue unveiling. I help to pay Dizzie's salary, so I don't see why I should not get a wrinkle from that artful dodger.

I plead guilty to having accepted the Birmingham invitation.* I thought they deserved to be encouraged for having asked a man of science to do the job instead of some noble swell; and, moreover, Satan whispered that it would be a good opportunity for a little ventilation of wickedness. I cannot say, however, that I can work myself up into much enthusiasm for the dry old Unitarian who did not go very deep into anything. But I think I may make him a good peg whereon to hang a discourse on the tendencies of modern thought.

I was not at the Cambridge pow-wow—not out of prudence, but because I was not asked. I suppose that decent respect towards a secretary of the Royal Society was not strong enough to outweigh University objections to the incumbent of that office. It is well for me that I expect nothing from Oxford or Cambridge, having burned my ships so far as they were concerned long ago.

I sent your note on to Knowles as soon as it arrived, but I have heard nothing from him. I wrote to him again to-night to say that he had better let me see it in proof if he is going to print it. I am right glad you find anything worth reading again in my old papers. I stand by the view I took of the origin of species now as much as ever.

Shall I not see the address? 'It is tantalising to hear of your progress, and not to know what is in it.

I am thinking of taking Development for the subject of my evening lecture,† the concrete facts made out in the last thirty years without reference to Evolution. If people see that it is Evolution, that is Nature's fault, and not mine.

We are all flourishing, and send our love.—Ever yours faithfully,
T. H. HUXLEY.

* To unveil the statue of Joseph Priestley. See page 439.

† *i.e.* At the British Association, he actually took "Animals as Automata."

terialists, for I am utterly incapable of conceiving the existence of matter if there is no mind in which to picture that existence; not among atheists, for the problem of the ultimate cause of existence is one which seems to me to be hopelessly out of reach of my poor powers. Of all the senseless babble I have ever had occasion to read, the demonstrations of these philosophers who undertake to tell us all about the nature of God would be the worst; if they were not surpassed by the still greater absurdities of the philosophers who try to prove that there is no God.

This essay was delivered as an evening address on August 24, the Monday of the Association week. A vast stir had been created by the treatment of deep reaching problems in Professor Tyndall's presidential address; interest was still further excited by this unexpected excursion into metaphysics. "I remember," writes Sir M. Foster, "having a talk with him about the lecture before he gave it. I think I went to his lodgings—and he sketched out what he was going to say. The question was whether, in view of the Tyndall row, it was wise in him to take the line he had marked out. In the end I remember his saying, 'Grasp your nettle, that is what I have got to do.'" But apart from the subject, the manner of the address struck the audience as a wonderful *tour de force*. The man who at first disliked public speaking, and always expected to break down on the platform, now, without note or reference of any kind, discoursed for an hour and a half upon a complex and difficult subject, in the very words which he had thought out and afterwards published.

This would have been a remarkable achievement if he had planned to do so and had learned up his speech; but the fact was that he was compelled to speak off-hand on the spur of the moment. He describes the situation in a letter of February 6, 1894, to Professor Ray Lankester:—

I knew that I was treading on very dangerous ground, so I wrote out uncommonly full and careful notes, and had them in my hand when I stepped on to the platform.

Then, I suddenly became aware of the bigness of the audience, and the conviction came upon me that, if I looked at my notes, not one half would hear me. It was a bad ten seconds,

as still as mice. There has been a great row about Tyndall's address, and I had some reason to expect that I should have to meet a frantically warlike audience. But it was quite otherwise, and though I spoke my mind with very great plainness I never had a warmer reception. And I am not without hope that I have done something to allay the storm, though, as you may be sure, I did not sacrifice plain speaking to that end. . . . I have been most creditably quiet here, and have gone to no dinners or breakfasts or other such fandangoes except those I accepted before leaving home. Sunday I spent quietly here, thinking over my lecture and putting my peroration, which required a good deal of care, into shape. I wandered out into the fields in the afternoon, and sat a long time thinking of all that had happened since I was here a young beginner, two and twenty, and . . . you were largely in my thoughts, which were full of blessings and tender memories.

I had a good night's work last night. I dined with the President of the College, then gave my lecture. After that I smoked a bit with Foster till eleven o'clock, and then I went to the *Northern Whig* office to see that the report of my lecture was all right. It is the best paper here, and the Editor had begged me to see to the report, and I was anxious myself that I should be rightly represented. So I sat there till a quarter past one having the report read and correcting it when necessary. Then I came home and got to bed about two. I have just been to the section and read my paper there to a large audience who cannot have understood ten words of it, but who looked highly edified, and now I have done. Our lodging has turned out admirably, and Ball's company has been very pleasant. So that the fiasco of our arrangements was all for the best.

I take the account of this last mentioned paper in Section D from the report in *Nature*:—

Professor Huxley opened the last day of the session with an account of his recent observations on the development of the *Columella auris* in Amphibia. (He described it as an outgrowth of the periotic capsule, and therefore unconnected with any visceral arch). . . .

In the absence of Mr. Parker there was no one competent to criticise the paper from personal knowledge; but a word dropped as to the many changes in the accepted homologies of the ossicula auditus, elicited a masterly and characteristic exposition of the series of new facts, and the modifications of the

violence." Later, "I am benevolent to all the world, being possessed of a dozen live axolotls and four or five big dead mesobranchs. Moreover, I am going to get endless Frogs and Toads by judicious exchange with Gunther. We will work up the Amphibia as they have not been done since they were crea—I mean evolved." *

The question of the pedicle comes up again when he simplifies some of Parker's results as to the development of the *Columella auris* in the Frog. "Your suprahyomandibular is nothing but the pedicle of the suspensorium over again. It has nothing whatever to do with the columella auris. . . . The whole thing will come out as simply as possible without any of your coalescences and combotherations. How you will hate me and the pedicle."

Tracing the development of the *columella* was a long business, but it grew clearer as young frogs of various ages were examined. "Don't be aggravated with yourself," he writes to Parker in July, "it's tough work, this here Frog." And on August 5: "I have worked over Toad and I have worked over Frog, and I tell an obstinate man that s.h.m. (suprahyomandibular) is a figment—or a vessel, whichever said obstinate man pleases." The same letter contains what he calls his final views on the *columella*, but by the end of the year he has gone further, and writes:—

Be prepared to bust-up with all the envy of which your malignant nature is capable. The problem of the vertebrate skull is solved. Fourteen segments or thereabouts in *Amphioxus*; all but one (barring possibilities about the ear capsule) aborted in higher vertebrata. Skull and brain of *Amphioxus* shut up like an opera-hat in higher vertebrata. So! (Sketch in illustration).

P.S.—I am sure you will understand the whole affair from this. Probably published it already in *Nature*!

A letter to the *Times* of July 8, 1874, on women's education, was evoked by the following circumstances. Miss Jex Blake's difficulties in obtaining a medical education

* Dr. A. C. L. G. Gunther, of the British Museum, where he was appointed Keeper of the Department of Zoology in 1875.

hardness of the times being considered—to be satisfied with fewer than seven members thereof.

I hear excellent accounts of the progress of the Station from Lankester, and I hope that it is now set on its legs permanently. As for the English contribution, you must look upon it simply as the expression of the hearty goodwill of your many friends in the land of fogs, and of our strong feeling that where you had sacrificed so much for the cause of science, we were, as a matter of duty,—quite apart from goodwill to you personally—bound to do what we could, each according to his ability.

Darwin is, in all things, noble and generous—one of those people who think it a privilege to let him help. I know he was very pleased with what you said to him. He is working away at a new edition of the *Descent of Man*, for which I have given him some notes on the brain question.

And apropos of that how is your own particular brain? I back la belle M—— against all the physicians in the world—even against mine own particular Æsculapius, Dr. Clark—to find the sovereignest remedy against the blue devils.

Let me hear from you—most abominable of correspondents as I am. And why don't you send Madame's photograph that you have promised?—Ever yours very faithfully,

T. H. HUXLEY.

Pray give my kind remembrances to your father.

4 MARLBOROUGH PLACE, *March* 31, 1874.

MY DEAR DARWIN—The brain business * is more than half done, and I will soon polish it off and send it to you. We are going down to Folkestone for a week on Thursday, and I shall take it with me.

I do not know what is doing about Dohrn's business at present. Foster took it in hand, but the last time I heard he was waiting for reports from Dew and Balfour.

You have been very generous as always; and I hope that other folk may follow your example, but like yourself I am not sanguine.

I have had an *awfully* tempting offer to go to Yankee-land on a lecturing expedition, and I am seriously thinking of making an experiment next spring.

* A note on the brain in man and the apes for the second edition of the *Descent of Man*.

attended séances and met mediums, by whom he was most unfavourably impressed.

Moreover, when invited to join a committee of investigation into spiritualistic manifestations, he replied:—

I regret that I am unable to accept the invitation of the Committee of the Dialectical Society to co-operate with a committee for the investigation of "Spiritualism"; and for two reasons. In the first place, I have not time for such an inquiry, which would involve much trouble and (unless it were unlike all inquiries of that kind I have known) much annoyance. In the second place, I take no interest in the subject. The only case of "Spiritualism" I have had the opportunity of examining into for myself, was as gross an imposture as ever came under my notice. But supposing the phenomena to be genuine—they do not interest me. If anybody would endow me with the faculty of listening to the chatter of old women and curates in the nearest cathedral town, I should decline the privilege, having better things to do. And if the folk in the spiritual world do not talk more wisely and sensibly than their friends report them to do, I put them in the same category. The only good that I can see in the demonstration of the truth of "Spiritualism" is to furnish an additional argument against suicide. Better live a crossing-sweeper than die and be made to talk twaddle by a "medium" hired at a guinea a *séance*.*

To the report above mentioned, Prof. G. Darwin, who also was present, added one or two notes and corrections.

REPORT ON SÉANCE

Jan. 27, 1874.

We met in a small room at the top of the house with a window capable of being completely darkened by a shutter and curtains opposite the door. A small light table with two flaps and four legs, unsteady and easily moved, occupied the middle of the room, leaving not much more than enough space for the chairs at the sides. There was a chair at each end, two chairs on the fireplace side, and one on the other. Mr. X (the medium) was seated in the chair at the door end, Mr. Y (the host) in

* Quoted from a review in the *Daily News*, October 17, 1871, of the Report on Spiritualism of the Committee of the London Dialectical Society.

we are going to have some manifestations." Immediately followed a noise obviously produced by the tumbling over of the accordion and some shifting of the position of the guitar. Next came a twanging—very slight, but of course very audible—of some of the strings, during which B was invisible. By and by B and A became visible again, and Medium's voice likewise showed that he had got back to his first position. But after he had returned to this position there was a noise of the guitar and other things on the table being stirred, and creeping noises like something light moving over the table. But no more actual twanging.

To my great disgust G. D. now began to remark that he saw two spots of light, which I suppose must have had the same origin as my rays A and B, and, moreover, that something occasionally occulted one or other of them. (Note: No, not till we changed places, G. H. D.) I blessed him for spoiling my game, but the effect was excellent. Nothing more happened. By and by, after some talk about these points of light, the medium suggested that this light was distracting, and that we had better shut it out. The suggestion was very dexterously and indirectly made, and was caught up more strongly (I think by Mr. Z). Anyhow, we agreed to stop out all light. The circle was broken, and the candle was lighted for this purpose. I then took occasion to observe that the guitar was turned round into the position noted in the margin, the end being near my left hand. On examining it I found a longish end of one of the catgut strings loose, and I found that by sweeping this end over the strings I could make quite as good twangs as we heard. I could have done this just as well with my mouth as with my hand—and I could have pulled the guitar about by the end of the catgut in my mouth and so have disturbed the other things—as they were disturbed.

Before the candle was lighted some discussion arose as to why the spirits would not do any better (started by Mr. Y and Mr. Z, I think), in which the medium joined. It appeared that (in the opinion of the spirits as interpreted by the medium) we were not quite rightly placed. When the discussion arose I made a bet with myself that the result would be that either I or G. D. would have to change places with somebody else. And I won my wager (I have just paid it with the remarkably good cigar I am now smoking). G. D. had to come round to my side, Mr. Z went to the end, and Mr. Y took G. D.'s place. "Good, Medium," said I to myself. "Now we shall see some-

lieved in the evidence of anyone with such perfect *bona fides* as Mr. Y being so worthless.

On receiving this report Mr. Darwin wrote (*Life*, ii. p. 188):—

Though the séance did tire you so much it was, I think, really worth the exertion, as the same sort of things are done at all the séances and now to my mind an enormous weight of evidence would be requisite to make me believe in anything beyond mere trickery.

The following letter to Mr. Morley, then editor of the *Fortnightly Review*, shows that my father was already thinking of writing upon Hume, though he did not carry out this intention till 1878.

The article referred to in the second letter is that on animals as automata.

4 MARLBOROUGH PLACE, N.W., *June* 4, 1874.

MY DEAR MR. MORLEY—I assure you that it was a great disappointment to me not to be able to visit you, but we had an engagement of some standing for Oxford.

Hume is frightfully tempting—I thought so only the other day when I saw the new edition advertised—and now I would gladly write about him in the *Fortnightly* if I were only sure of being able to keep any engagement to that effect I might make.

But I have yet a course of lectures before me, and an evening discourse to deliver at the British Association—to say nothing of opening the Manchester Medical School in October—and polishing off a lot of scientific work. So you see I have not a chance of writing about Hume for months to come, and you had much better not trust to such a very questionable reed as I am.—Ever yours very faithfully,

T. H. HUXLEY.

4 MARLBOROUGH PLACE, N.W., *Nov.* 15, 1874.

MY DEAR MORLEY—Many thanks for your abundantly sufficient cheque—rather too much, I think, for an article which had been gutted by the newspapers.

I am always very glad to have anything of mine in the *Fortnightly*, as it is sure to be in good company; but I am becoming as spoiled as a maiden with many wooers. However, as far as the *Fortnightly* which is my old love, and the *Contemporary*

Thus the skull of *Petromyzon* answers to about fourteen segments of the body of *Amphioxus*, fused together and indistinguishable in even the earliest embryonic state of the higher vertebrata.

Does this take your breath away? Well, in due time you shall be convinced. I sent in a brief notice to the last meeting of the Royal Society, which will soon be in your hands.

I need not tell you of the importance of all this. It is unlucky for Semper that he has just put *Amphioxus* out of the *Vertebrata* altogether—because it is demonstrable that *Amphioxus* is nearer than could have been hoped to the condition of the primitive vertebrate—a far more regular and respectable sort of ancestor than even you suspected. For you see “Acrania” will have to go.

I think we must have an English translation of the *Anthropogenie*. There is great interest in these questions now, and your book is very readable, to say nothing of its higher qualities.

My wife (who sends her kindest greetings) and I were charmed with the photograph. [As for our] publication in that direction, the seven volumes are growing into stately folios. You would not know them.—Ever yours very faithfully,

T. H. HUXLEY.

How will you read this scrawl now that Gegenbaur is gone?

In the article here referred to, a review of a book by Prof. G. H. Darwin, a personal attack of an unjustifiable character was made upon him, and through him, upon Charles Darwin. The authorship of the review in question had come to be known, and Huxley writes to his friend:—

I entirely sympathise with your feeling about the attack on George. If anybody tries that on with my boy L., the old wolf will show all the fangs he has left by that time, depend upon it. . . .

You ought to be like one of the blessed gods of Elysium, and let the inferior deities do battle with the infernal powers. Moreover, the severest and most effectual punishment for this sort of moral assassination is quietly to ignore the offender and give him the cold shoulder. He knows why he gets it, and society comes to know why, and though society is more or less of a dunderhead it has honourable instincts, and the man in the cold finds no cloak that will cover him.

It is, I hope, unnecessary for me to give a formal contradiction to the silly fiction, which is assiduously circulated by the fanatics who not only ought to know, but do know, that their assertions are untrue, that I have advocated the introduction of that experimental discipline which is absolutely indispensable to the professed physiologist, into elementary teaching.

Moreover, during the debates on the Vivisection Bill in 1876, the late Lord Shaftesbury made use of this story. Huxley was extremely indignant, and wrote home:—

Did you see Lord Shaftesbury's speech in Tuesday's *Times*? I saw it by chance,* and have written a sharp letter to the *Times*.

This letter appeared on May 26, when he wrote again:—

You will have had my note, and know all about Lord Shaftesbury and his lies by this time. Surely you could not imagine on any authority that I was such an idiot as to recommend boys and girls to perform experiments which are difficult to skilled anatomists, to say nothing of other reasons.

LETTER TO THE *TIMES*

In your account of the late debate in the House of Lords on the Vivisection Bill, Lord Shaftesbury is reported to have said that in my *Lessons in Elementary Physiology*, it is strongly insisted that such experiments as those subjoined shall not merely be studied in the manual, but actually repeated, either by the boys and girls themselves or else by the teachers in their presence, as plainly appears from the preface to the second edition.

I beg leave to give the most emphatic and unqualified contradiction to this assertion, for which there is not a shadow of justification either in the preface to the second edition of my *Lessons* or in anything I have ever said or written elsewhere. The most important paragraph of the preface which is the subject of Lord Shaftesbury's misquotation and misrepresentation stands as follows:—

“For the purpose of acquiring a practical, though elementary, acquaintance with physiological anatomy and his-

* Being in Edinburgh, he had been reading the Scotch papers, and “the reports of the Scotch papers as to what takes place in Parliament are meagre.”

which is attainable by mere reading, though infinitely better than ignorance, is knowledge of a very different kind from that which arises from direct contact with fact."

"Direct contact with fact!" What can that mean (so, at least, very many ask) but a declaration, on high authority, to teachers and learners that vivisection alone can give them any real and effective instruction?

But the subsequent passage is still stronger, for it states "that the worth of the pursuit of science, as an intellectual discipline, is almost lost by those who only seek it in books."

Is not language like this calculated to touch the zeal and vanity of teachers and learners at the very quick, and urge them to improve their own minds and stand well in the eyes of the profession and the public by positive progress in experimental physiology? Ordinary readers, most people would think, could come to no other conclusion.

But a disclaimer from Professor Huxley is enough; I am sorry to have misunderstood him; and I must ask his pardon. I sincerely rejoice to have received such an assurance that his great name shall never be used for such a project as that which excited our fears.

On this he wrote:—

You will have seen Lord Shaftesbury's reply to my letter. I thought it frank and straightforward, and I have written a private letter * to the old boy of a placable and proper character.

In 1874 he had also had a small passage of arms with the late Mr. W. E. Forster, then Vice-President of the Council, upon the same subject. Mr. Forster was about to leave office, and when he gave his official authorisation for summer courses of lectures at South Kensington on Biology, Chemistry, Geology, etc., he did so with the special proviso that there be no vivisection experiments in any of the courses, and further, appended a Memorandum, explaining the reasons on which he acted.

Now, although Huxley was mentioned by name as having taken care to avoid inflicting pain in certain previous

* "Huxley, the Professor, has written me a very civil, nay kind, letter. I replied in the same spirit." (Lord Shaftesbury, *Life and Work*, iii. 373, June 3, 1876.)

fended Brown Séquard, that I might expect to meet with every description of abuse and misrepresentation if such demonstrations were given.

It did not appear to me, however, that the latter consideration ought to weigh with me, and I took such a course as I believe is defensible against everything but misrepresentation.

I gave strict instructions to the Demonstrators who assisted me that no such experiments were to be performed, unless the animal were previously rendered insensible to pain either by destruction of the brain or by the administration of anæsthetics, and I have every reason to believe that my instructions were carried out. I do not see what I can do beyond this, or how I can give Mr. Forster any better guarantee than is given in my assurance that my dislike to the infliction of pain both as a matter of principle and of feeling is quite as strong as his own can be.

If Mr. Forster is not satisfied with this assurance, and with its practical result that our experiments are made only on non-sentient animals, then I am afraid that my position as teacher of Physiology must come to an end.

If I am to act in that capacity I cannot consent to be prohibited from showing the circulation in a frog's foot because the frog is made slightly uncomfortable by being tied up for that purpose; nor from showing the fundamental properties of nerves, because extirpating the brain of the same animal inflicts one-thousandth part of the prolonged suffering which it undergoes when it makes its natural exit from the world by being slowly forced down the throat of a duck, and crushed and asphyxiated in that creature's stomach.

I shall be very glad to wait upon Mr. Forster if he desires to see me. Of course I am most anxious to meet his views as far as I can, consistently with my position as a person bound to teach properly any subject in which he undertakes to give instruction. But I am quite clear as to the amount of freedom of action which it is necessary I should retain, and if you will kindly communicate the contents of this letter to the Vice-President of the Council, he will be able to judge for himself how far his sense of what is right will leave me that freedom, or render it necessary for me to withdraw from what I should regard as a false position.

But there was a further and more vital question. He had already declared through Major (now Sir John) Don-

However, I will not trouble your Lordship with any further indication of the difficulties which, as I imagine, will attend the attempt to carry the Minute into operation, if instruction is to be given in Physiology, or even in general Biology.

The upshot of the matter was that the Minute was altered so as to refer solely to future courses, and on February 20 he wrote to Mr. Forster:—

I cannot allow you to leave office without troubling you with the expression of my thanks for the very great kindness and consideration which I have received from you on all occasions, and particularly in regard to the question of vivisection, on which I ventured to some extent, though I think not very widely or really, to differ from you.

The modification which you were good enough to make in your minute removed all my objections to undertaking the Summer Course.

And I am sure that if that course had happened to be a physiological one I could do all I want to do in the way of experiment, without infringing the spirit of your minute, though I confess that the letter of it would cause me more perplexity.

As to his general attitude to the subject, it must be noted, as said above in the letter to Sir J. Donnelly, that he never followed any line of research involving experiments on living and conscious animals. Though, as will be seen from various letters, he considered such experiments justifiable, his personal feelings prevented him from performing them himself. Like Charles Darwin, he was very fond of animals, and our pets in London found in him an indulgent master.

But if he did not care to undertake such experiments personally, he held it false sentiment to blame others who did disagreeable work for the good of humanity, and false logic to allow pain to be inflicted in the cause of sport while forbidding it for the cause of science. (See his address on "Instruction in Elementary Physiology," *Coll. Essays*, iii. 300 *seq.*) Indeed, he declared that he trusted to the fox-hunting instincts of the House of Commons rather than to any real interest in science in that body, for a moderate treatment of the question of vivisection.

and Pharmacology, and therefore to the progress of rational medicine.

Another letter on the subject may be given, which was written to a student at a theological college, in reply to a request for his opinion on vivisection, which was to be discussed at the college debating society.

GRAND HOTEL, EASTBOURNE, *Sept.* 29, 1890.

DEAR SIR—I am of the opinion that the practice of performing experiments on living animals is not only reconcilable with true humanity, but under certain circumstances is imperatively demanded by it.

Experiments on living animals are of two kinds. First, those which are made upon animals which, although living, are incapable of sensation, in consequence of the destruction or the paralysis of the sentient machinery.

I am not aware that the propriety of performing experiments of this kind is seriously questioned, except in so far as they may involve some antecedent or subsequent suffering. Of course those who deny that under any circumstances it can be right to inflict suffering on other sentient beings for our own good, must object to even this much of what they call cruelty. And when they prove their sincerity by leaving off animal food; by objecting to drive castrated horses, or indeed to employ animal labour at all; and by refusing to destroy rats, mice, fleas, bugs and other sentient vermin, they may expect sensible people to listen to them, and sincere people to think them other than sentimental hypocrites.

As to experiments of the second kind, which do not admit of the paralysis of the sentient mechanism, and the performance of which involves severe prolonged suffering to the more sensitive among the higher animals, I should be sorry to make any sweeping assertion. I am aware of a strong personal dislike to them, which tends to warp my judgment, and I am prepared to make any allowance for those who, carried away by still more intense dislike, would utterly prohibit these experiments.

But it has been my duty to give prolonged and careful attention to this subject, and putting natural sympathy aside, to try and get at the rights and wrongs of the business from a higher point of view, namely, that of humanity, which is often very different from that of emotional sentiment.

I ask myself—suppose you knew that by inflicting prolonged

4 MARLBOROUGH PLACE, *Jan. 22, 1875.*

MY DEAR DARWIN—I quite agree with your letter about vivisection as a matter of right and justice in the first place, and secondly as the best method of taking the wind out of the enemy's sails. I will communicate with Burdon Sanderson and see what can be done.

My reliance as against — and her fanatical following is not in the wisdom and justice of the House of Commons, but in the large number of fox-hunters therein. If physiological experimentation is put down by law, hunting, fishing and shooting, against which a much better case can be made out, will soon follow.—Ever yours, very faithfully,

T. H. HUXLEY.

SOUTH KENSINGTON, *April 21, 1875.*

MY DEAR DARWIN—The day before yesterday I met Playfair at the club, and he told me that he had heard from Miss Elliott that *I* was getting up what she called a "Vivisector's Bill," and that Lord Cardwell was very anxious to talk with some of us about the matter.

So you see that there is no secret about our proceedings. I gave him a general idea of what was doing, and he quite confirmed what Lubbock said about the impossibility of any action being taken in Parliament this session.

Playfair said he should like very much to know what we proposed doing, and I should think it would be a good thing to take him into consultation.

On my return I found that Pflüger had sent me his memoir with a note such as he had sent to you.

I read it last night, and I am inclined to think that it is a very important piece of work.

He shows that frogs absolutely deprived of oxygen give off carbonic acid for twenty-five hours, and gives very strong reasons for believing that the evolution of carbonic acid by living matter in general is the result of a process of internal rearrangement of the molecules of the living matter, and not of direct oxidation.

His speculations about the origin of living matter are the best I have seen yet, so far as I understand them. But he plunges into the depths of the higher chemistry in which I am by no means at home. Only this I can see, that the paper is worth careful study.—Ever yours faithfully,

T. H. HUXLEY.

I am very busy with my lectures, and am nearly half through. I shall not be sorry when they are over, as I have been grinding away now since last October.—With kindest regards to Mrs. Darwin, ever yours very faithfully,

T. H. HUXLEY.

He was duly asked to serve on the Commission. Though his lectures in Edinburgh prevented him from attending till the end of July no difficulty was made over this, as the first meetings of the Commission, which began on June 30, were to be devoted to taking the less controversial evidence. In accepting his nomination he wrote to Mr. Cross (afterwards Lord Cross), at that time Home Secretary:—

If I can be of any service I shall be very glad to act on the Commission, sympathising as I do on the one hand with those who abhor cruelty to animals, and, on the other, with those who abhor the still greater cruelty to man which is involved in any attempt to arrest the progress of physiology and of rational medicine.

The other members of the Commission were Lords Cardwell and Winmarleigh, Mr. W. E. Forster, Sir J. B. Karlake, Professor Erichssen, and Mr. R. H. Hutton.

The evidence given before the Commission bore out the view that English physiologists inflicted no more pain upon animals than could be avoided; but one witness, not an Englishman, and not having at that time a perfect command of the English language, made statements which appeared to the Commission at least to indicate that the witness was indifferent to animal suffering. Of this incident Huxley writes to Mr. Darwin at the same time as he forwarded a formal invitation for him to appear as a witness before the Commission:—

4 MARLBOROUGH PLACE, *Oct.* 30, 1875.

MY DEAR DARWIN—The inclosed tells its own story. I have done my best to prevent your being bothered, but for various reasons which will occur to you I did not like to appear too obstructive, and I was asked to write to you. The strong feeling of my colleagues (and my own I must say also) is that we ought to have your opinions in our minutes. At the same time there is a no less strong desire to trouble you as little as possible, and under no circumstances to cause you any risk of injury to health.

the strength of which legislation was recommended went beyond the facts, the report went beyond the evidence, the recommendations beyond the report, and the bill can hardly be said to have gone beyond the recommendations, but rather to have contradicted them."

As to the working of the law Huxley referred to it the following year in the address, already cited, on "Elementary Instruction in Physiology" (*Coll. Essays*, iii. 310).

But while I should object to any experimentation which can justly be called painful, and while as a member of a late Royal Commission I did my best to prevent the infliction of needless pain for any purpose, I think it is my duty to take this opportunity of expressing my regret at a condition of the law which permits a boy to troll for pike or set lines with live frog bait for idle amusement, and at the same time lays the teacher of that boy open to the penalty of fine and imprisonment if he uses the same animal for the purpose of exhibiting one of the most beautiful and instructive of physiological spectacles—the circulation in the web of the foot. No one could undertake to affirm that a frog is not inconvenienced by being wrapped up in a wet rag and having his toes tied out, and it cannot be denied that inconvenience is a sort of pain. But you must not inflict the least pain on a vertebrated animal for scientific purposes (though you may do a good deal in that way for gain or for sport) without due licence of the Secretary of State for the Home Department, granted under the authority of the Vivisection Act.

So it comes about that, in this year of grace 1877, two persons may be charged with cruelty to animals. One has impaled a frog, and suffered the creature to writhe about in that condition for hours; the other has pained the animal no more than one of us would be pained by tying strings round his fingers and keeping him in the position of a hydropathic patient. The first offender says, "I did it because I find fishing very amusing," and the magistrate bids him depart in peace—nay, probably wishes him good sport. The second pleads, "I wanted to impress a scientific truth with a distinctness attainable in no other way on the minds of my scholars," and the magistrate fines him five pounds.

I cannot but think that this is an anomalous and not wholly creditable state of things.

pointed. But he found the work to be unexpectedly hard, and very soon he had the sense of panting to keep pace with the demands of the lecturer. It was not merely that the texture of scientific reasoning in the lectures was so closely knit,—although that was a very palpable fact,—but the character of Huxley's terminology was entirely strange to him. It met him on his weakest side, for it presupposed a knowledge of Greek (being little else than Greek compounds with English terminations) and of Greek he had none.

Huxley's usual lectures, he writes, are something awful to listen to. One half of the class, which numbers about four hundred, have given up in despair from sheer inability to follow him. The strain on the attention of each lecture is so great as to be equal to any ordinary day's work. I feel quite exhausted after them. And then to master his language is something dreadful. But, with all these drawbacks, I would not miss them, even if they were ten times as difficult. They are something glorious, sublime!

Again he writes:—

Huxley is still very difficult to follow, and I have been four times in his lectures completely stuck and utterly helpless. But he has given us eight or nine beautiful lectures on the frog. . . . If you only heard a few of the lectures you would be surprised to find that there were so few missing links in the chain of life, from the *amœba* to the genus *homo*.

It was a large class, ultimately reaching 353 and breaking the record of the Edinburgh classes without having recourse to the factitious assistance proposed in the letter of May 16.

His inaugural lecture was delivered under what ought to have been rather trying circumstances. On the way from London he stopped a night with his old friends, John Bruce and his wife (one of the Fannings), at their home, Barmoor Castle, near Beal. He had to leave at 6 next morning, reaching Edinburgh at 10, and lecturing at 2. "Nothing," he writes, "could be much worse, but I am going through it with all the cheerfulness of a Christian martyr."

On May 3 he writes to his wife from the Bruces' Edinburgh house, which they had lent him.

TO HIS ELDEST DAUGHTER

EDINBURGH, *May* 16, 1875.

MY DEAREST JESS—Your mother's letter received this morning reminds me that I have not written to "Cordelia" (I suppose she means Goneril) by a message from that young person—so here is reparation.

I have 330 students, and my class is the biggest in the University—but I am quite cast down and discontented because it is not 351,—being one more than the Botany Class last year—which was never so big before or since.

I am thinking of paying 21 street boys to come and take the extra tickets so that I may crow over all my colleagues.

Fanny Bruce is going to town next week to her grandmother's and I want you girls to make friends with her. It seems to me that she is very nice—but that is only a fallible man's judgment, and Heaven forbid that I should attempt to forestall Miss Cudberry's decision on such a question. Anyhow she has plenty of energy and, among other things, works very hard at *German*.

M—— says that the Rootle-Tootles have a bigger drawing-room than ours. I should be sorry to believe these young beginners guilty of so much presumption, and perhaps you will tell them to have it made smaller before I visit them.

A Scotch gentleman has just been telling me that May is the worst month in the year, here; so pleasant! but the air is soft and warm to-day, and I look out over the foliage to the castle and don't care.

Love to all, and specially M——. Mind you don't tell her that I dine out to-day and to-morrow—positively for the first and last times.—Ever your loving father,

T. H. HUXLEY.

However, the class grew without such adventitious aid, and he writes to Mr. Herbert Spencer on June 15:—

. . . I have a class of 353, and instruct them in dry facts—particularly warning them to keep free of the infidel speculations which are current under the name of evolution.

I expect an "examiner's call" from a Presbytery before the course is over, but I am afraid that the pay is not enough to induce me to forsake my "larger sphere of influence" in London.

How is it that Dohrn has been and gone? I have been meditating a letter to him for an age. He wanted to see me, and I did not know how to manage to bring about a meeting.

Edinburgh is greatly exercised in its mind about the vivisection business, and "Vagus" "swells wisely" whenever the subject is mentioned. I think there is an inclination to regard those who are ready to consent to legislation of any kind as traitors, or, at any rate, trimmers. It sickens me to reflect on the quantity of time and worry I shall have to give to that subject when I get back.

I see that — has been blowing the trumpet at the Medical Association. He has about as much tact as a flyblown bull.

I have just had a long letter from Wyville Thomson. The *Challenger* inclines to think that *Bathybius* is a mineral precipitate! in which case some enemy will probably say that it is a product of my precipitation. So mind, I was the first to make that "goak." Old Ehrenberg suggested something of the kind to me, but I have not his letter here. I shall eat my leek handsomely, if any eating has to be done. They have found pseudopodia in *Globigerina*.

With all good wishes from ours to yours—Ever yours faithfully,
T. H. HUXLEY.

CRAGSIDE, MORPETH, *August* 13, 1875.

MY DEAR TYNDALL—I find that in the midst of my work in Edinburgh I omitted to write to De Vrij, so I have just sent him a letter expressing my pleasure in being able to co-operate in any plan for doing honour to old Benedict,* for whom I have a most especial respect.

I am not sure that I won't write something about him to stir up the Philistines.

My work at Edinburgh got itself done very satisfactorily, and I cleared about £1000 by the transaction, being one of the few examples known of a Southern coming north and pillaging the Scots. However, I was not sorry when it was all over, as I had been hard at work since October and began to get tired.

The wife and babies from the south, and I from the north, met here a fortnight ago and we have been idling very pleasantly ever since. The place is very pretty and our host kindness itself. Miss Matthaei and five of the bairns are at Cartington—a moorland farm-house three miles off—and in point of rosy

* Spinoza, a memorial to whom was being raised in Holland.

I assure you I have not forgotten the brief interview to which you refer, and I have often regretted that the hurry and worry of life (which increases with the square of your distance from youth) never allowed me to take advantage of your kind father's invitation to become better acquainted with him and his. I found his card in Jermyn Street when I returned last year, with a pencilled request that I would call on him at Westminster.

I meant to do so, but the whirl of things delayed me until, as I bitterly regret, it was too late.

I am not sure that I have any important letter of your father's but one, written to me some fifteen years ago, on the occasion of the death of a child who was then my only son. It was in reply to a letter of my own written in a humour of savage grief. Most likely he burned the letter, and his reply would be hardly intelligible without it. Moreover, I am not at all sure that I can lay my hands upon your father's letter in a certain chaos of papers which I have never had the courage to face for years. But if you wish I will try.

I am very grieved to hear of Mrs. Kingsley's indisposition. Pray make my kindest remembrances to her, and believe me yours very faithfully,

T. H. HUXLEY.

P.S.—By the way, letters addressed to my private residence,

4 MARLBOROUGH PLACE, N.W.,

are sure not to be delayed. And I have another reason for giving the address—the hope that when you come to Town you will let my wife and daughters make your acquaintance.

His continued interest in the germ-theory and the question of the origin of life (*Address at the British Association, 1870, see p. 355, sq.*), appears from the following:—

4 MARLBOROUGH PLACE, Oct. 15, 1875.

MY DEAR TYNDALL—Will you bring with you to the x tomorrow a little bottle full of fluid containing the bacteria you have found developed in your infusions? I mean a good characteristic specimen. It will be useful to you, I think, if I determine the forms with my own microscope, and make drawings of them which you can use.—Ever yours,

T. H. HUXLEY.

I can't tell you how delighted I was with the experiments.

of original work to settle questions which have been hanging vaguely in my mind for years. If *Amphibia* is done by the end of January it is as much as it will be.

In February I must give myself—or at any rate my spare self—up to my Rectorial Address,* which (tell it not in Gath) I wish at the bottom of the Red Sea. And I do not suppose I shall be able to look seriously at either *Animal Kingdom* or *Anthropology* before the address is done with. And all depends on the centre of my microcosm—intestinum colon—which plays me a trick every now and then.

I will do what I can if you like, but if you trust me it is at your proper peril.

Feb. 8, 1874.—How astonished folks will be if eloquent passages out of the address get among the *Amphibia*, and comments on Frog anatomy into the address. As I am working at both just now this result is not improbable.

Meanwhile the address and the ten days' stay at Aberdeen had been "playing havoc with the *Amphibia*," but on returning home, he went to work upon the latter, and writes on March 12:—

I did not care to answer your last letter until I had an instalment of *Amphibia* ready. Said instalment was sent off to you, care of Messrs. Black, yesterday, and now I feel like Dick Swiveller, when happy circumstances having enabled him to pay off an old score he was able to begin running up another.

June 8.—I have had sundry proofs and returned them. My writing is lamentable when I am in a hurry, but I never provoked a strike before! I declare I think I write as well as the editor, on ordinary occasions.

He was pleased to find someone who wrote as badly as, or worse than, himself, and several times rallies Baynes on that score. Thus, when Mrs. Baynes had acted as her husband's amanuensis, he writes (February 11, 1878):—

My respectful compliments to the "mere machine," whose beautiful caligraphy (if that isn't a tautology) leaves no doubt in my mind that whether the writing of your letters by that agency is good for you or not it is admirable for your correspondents.

Why people can't write a plain legible hand I can't imagine.*

* His Rectorial Address at Aberdeen. (See p. 436.)

† *N.B.*—This sentence is written purposely in a most illegible hand.

CRAGSIDE, MORPETH, *Aug. 24, 1875.*

MY DEAR BAYNES—I think —— is like enough to do the “*Cœlenterata*” well if you can make sure of his doing it at all. He is a man of really great knowledge of the literature of Zoology, and if it had not been for the accident of being a procrastinating impracticable ass, he could have been a distinguished man. But he is a sort of Balaam-Centaur with the asinine stronger than the prophetic moiety.

I should be disposed to try him, nevertheless.

I don't think I have had final revise of *Biology* yet.

I do not know that “*Cœlenterata*” is Lankester's specialty. However, he is sure to do it well if he takes it up.—Ever yours very faithfully,

T. H. HUXLEY.

4 MARLBOROUGH PLACE, N.W., *Oct. 12, 1875.*

MY DEAR BAYNES—Do you remember my telling you that I should before long be publishing a book, of which general considerations on *Biology* would form a part, and that I should have to go over the same ground as in the article for the *Encyclopædia*?

Well, that prediction is about to be verified, and I want to know what I am to do.

You see, as I am neither dealing with Theology, nor History, nor Criticism, I can't take a fresh departure and say something entirely different from what I have just written.

On the other hand, if I republish what stands in the article, the *Encyclopædia* very naturally growls.

What do the sweetest of Editors and the most liberal of Proprietors say ought to be done under the circumstances?

I pause for a reply.

I have carried about Stanley's* note in my pocket-book until I am sorry to say the flyleaf has become hideously stained.

The wife and daughters could make nothing of it, but I, accustomed to the MS. of certain correspondents, have no doubt as to the fourth word of the second sentence. It is “*Canterbury*.”† Nothing can be plainer.

Hoping the solution is entirely satisfactory,—Believe me, ever yours very faithfully,

T. H. HUXLEY.

* The Dean's handwriting was proverbial.

† The writing of this word is carefully slurred until it is almost as illegible as the original.

And on June 9:—

After nine days' meditation (directed exclusively to the Harvey and Hunter question) I am not any "forrarder," as the farmer said after his third bottle of Gladstone claret. So perhaps I had better mention the fact. I am very glad you have limed Flower for "Mammalia" and "Horse"—nobody could be better.

4 MARLBOROUGH PLACE, N.W., *July 1, 1879.*

MY DEAR BAYNES—On Thursday last I sought for you at the Athenæum in the middle of the day, and told them to let me know if you came in in the evening when I was there again. But I doubt not you were plunged in dissipation.

My demonstrator Parker showed me to-day a letter he had received from Black's, asking him to do anything in the small Zoology way between H and L.

He is a modest man, and so didn't ask what the H—L he was to do, but he looked it.

Will you enlighten him or me, and I will convey the information on?

I had another daughter married yesterday. She was a great pet and it is very hard lines on father and mother. The only consolation is that she has married a right good fellow, John Collier the artist.—Ever yours very faithfully,

T. H. HUXLEY.

July 19, 1879.—Many thanks for your and Mrs. Baynes' congratulations. I am very well content with my son-in-law, and have almost forgiven him for carrying off one of my pets, which shows a Christian spirit hardly to be expected of me.

SOUTH KENSINGTON, *July 2, 1880.*

MY DEAR BAYNES—I have been thinking over the matter of Instinct, and have come to the conclusion that I dare not undertake anything fresh.

There is an address at Birmingham in the autumn looming large, and ghosts of unfinished work flitter threateningly.—Ever yours very faithfully,

T. H. HUXLEY.

two sets of facts flow from a common cause, *évolution*. Descent by modification accounts for similarity of structure; the process of gradual adaptation to conditions accounts for the existing adaptation to purpose. To be a teleologist and yet accept evolution it is only necessary "to suppose that the original plan was sketched out—that the purpose was foreshadowed in the molecular arrangements out of which the animals have come."

This was no new view of his. While, ever since his first review of the *Origin* in 1859 (*Coll. Ess.* ii. 6), he had declared the commoner and coarser forms of teleology to find their most formidable opponent in the theory of evolution, and in 1869, addressing the Geological Society, had spoken of "those final causes, which have been named barren virgins, but which might be more fitly termed the *hctairæ* of philosophy, so constantly have they led men astray" (*ib.* viii. 80; cp. ii. 21, 36), he had, in his *Criticism of the Origin* (1864, ii. 86), and the *Genealogy of Animals* (1869, ii. 109, *sqq.*), shown "how perhaps the most remarkable service to the philosophy of Biology rendered by Mr. Darwin is the reconciliation of teleology and morphology, and the explanation of the facts of both which his views offer . . . the wider teleology, which is actually based upon the fundamental proposition of evolution."

His note-book shows that he was busy with *Reptilia* from Elgin and from India; and with his *Manual of Invertebrate Anatomy*, which was published the next year; while he refused to undertake a course of ten lectures at the Royal Institution, saying that he had already too much other work to do, and would have no time for original work.

About this time, also, in answer to a request from a believer in miracles, "that those who fail to perceive the cogency of the evidence by which the occurrence of miracles is supported, should not confine themselves to the discussion of general principles, but should grapple with some particular case of an alleged miracle," he read before the Metaphysical Society a paper dealing with the evidence for the miracle of the resurrection. (See p. 342.)

birthday to-morrow. Luckily there will be no more of it. Vanity of Vanities! Saturday afternoon I go out to Lord Young's place to spend Sunday. I have been in rather a hypochondriacal state of mind, and I will see if this course of medicine will drive the seven devils out.

One of the chief friendships which sprang from this residence in Edinburgh was that with Dr. (afterwards Sir John) Skelton, widely known under his literary pseudonym of "Shirley." A civil servant as well as a man of letters, he united practical life with literature, a combination that appealed particularly to Huxley, so that he was a constant visitor at Dr. Skelton's picturesque house, the Hermitage of Braid, near Edinburgh. A number of letters addressed to Skelton from 1875 to 1891 show that with him Huxley felt the stimulus of an appreciative correspondent.

4 MELVILLE STREET, EDINBURGH, *June 23, 1876.*

MY DEAR SKELTON—I do not understand how it is that your note has been so long in reaching me; but I hasten to repel the libellous insinuation that I have vowed a vow against dining at the Hermitage.

I wish I could support that repudiation by at once accepting your invitation for Saturday or Sunday, but my Saturdays and Sundays are mortgaged to one or other of your judges (good judges, obviously).

Shall you be at home on Monday or Tuesday? If so, I would put on a kilt (to be as little dressed as possible), and find my way out and back; happily improving my mind on the journey with the tracts you mention.—Ever yours very faithfully,

T. H. HUXLEY.

4 MELVILLE STREET, EDINBURGH, *July 1, 1876.*

MY DEAR SKELTON—Very many thanks for the copy of the *Comedy of the Noctes*, which reached me two or three days ago. Turning over the pages I came upon the Shepherd's "Terrible Journey of Timbuctoo," which I enjoyed as much as when I first read it thirty odd years ago.—Ever yours very faithfully,

T. H. HUXLEY.

On June 23 he writes home:—

Did you read Gilman's note asking me to give the inaugural discourse at the Johns Hopkins University, and offering £100 on

championship of the new doctrines had at the same time been a championship of freedom and sincerity in thought and word against shams and self-deceptions of every kind. It was not so much the preacher of new doctrines who was welcomed, as the apostle of veracity—not so much the student of science as the teacher of men.

Moreover, another sentiment coloured this holiday visit. He was to see again the beloved sister of his boyhood. She had always prophesied his success, and now after thirty years her prophecy was fulfilled by his coming, and, indeed, exceeded by the manner of it.

Mr. Smalley, then London correspondent of the *New York Tribune*, was a fellow passenger of his on board the *Germanic*, and tells an interesting anecdote of him:—

Mr. Huxley stood on the deck of the *Germanic* as she steamed up the harbour of New York, and he enjoyed to the full that marvellous panorama. At all times he was on intimate terms with Nature and also with the joint work of Nature and Man; Man's Place in Nature being to him interesting from more points of view than one. As we drew near the city—this was in 1876, you will remember—he asked what were the tall tower and tall building with a cupola, then the two most conspicuous objects. I told him the Tribune and the Western Union Telegraph buildings. “Ah,” he said, “that is interesting; that is American. In the Old World the first things you see as you approach a great city are steeples; here you see, first, centres of intelligence.” Next to those the tug-boats seemed to attract him as they tore fiercely up and down and across the bay. He looked long at them and finally said, “If I were not a man I think I should like to be a tug.” They seemed to him the condensation and complete expression of the energy and force in which he delighted.

The personal welcome he received from the friends he visited was of the warmest. On the arrival of the *Germanic* the travellers were met by Mr. Appleton the publisher, and carried off to his country house at Riverdale. While his wife was taken to Saratoga to see what an American summer resort was like, he himself went on the 9th to New Haven, to inspect the fossils at Yale College, collected from the Tertiary deposits of the Far West by Professor Marsh, with

A letter to his wife describes his visit to Yale:—

My excellent host met me at the station, and seems as if he could not make enough of me. I am installed in apartments which were occupied by his uncle, the millionaire Peabody, and am as quiet as if I were in my own house. We have had a preliminary canter over the fossils, and I have seen some things which were worth all the journey across.

This is the most charmingly picturesque town, with the streets lined by avenues of elm trees which meet overhead. I have never seen anything like it, and you must come and look at it. There is fossil work enough to occupy me till the end of the week, and I have arranged to go to Springfield on Monday to examine the famous footprints of the Connecticut Valley.

The Governor has called upon me, and I shall have to go and do pretty-behaved *chez lui* to-morrow. An application has come for an autograph, but I have not been interviewed!

This immunity, however, did not last long. He appears to have been caught by the interviewer the next day, for he writes on the 11th:—

[I have not seen the notice in the *World* you speak of. You will be amused at the article written by the interviewer. He was evidently surprised to meet with so little of the “highfalutin” philosopher in me, and says I am “affable” and of “the commercial or mercantile” type. That is something I did not know, and I am rather proud of it. We may be rich yet.]

As to his work at Yale Museum, he writes in the same letter:—

We are hard at work still. Breakfast at 8.30—go over to the Museum with Marsh at 9 or 10—work till 1.30—dine—go back to Museum to work till 6. Then Marsh takes me for a drive to see the views about the town, and back to tea about half-past eight. He is a wonderfully good fellow, full of fun and stories about his Western adventures, and the collection of fossils is the most wonderful thing I ever saw. I wish I could spare three weeks instead of one to study it.

[To-morrow evening we are to have a dinner by way of winding up, and he has asked a lot of notables to meet me. I assure you I am being “made of,” as I thought nobody but the little wife was foolish enough to do.]

Fiske, at their summer home. Among the other visitors were the eminent musical composer Mr. Paine, the poet Cranch, and daughters of Hawthorne and Longfellow, so that they found themselves in the midst of a particularly cheerful and delightful party. From Petersham they proceeded to Buffalo, the meeting-place that year of the American Association for the Advancement of Science, which my father had promised to attend. Here they stayed with Mr. Marshall, a leading lawyer, who afterwards visited them in England.

A week was spent at Niagara, partly in making holiday, partly in shaping the lectures which had to be delivered at the end of the trip. As to the impression made upon him by the Falls—an experience which, it is generally presumed, every traveller is bound to record—I may note that after the first disappointment at their appearance, inevitable wherever the height of a waterfall is less than the breadth, he found in them an inexhaustible charm and fascination. As in duty bound, he, with my mother, completed his experiences by going under the wall of waters to the “Cave of the Winds.” But of all things nothing pleased him more than to sit of an evening by the edge of the river, and through the roar of the cataract to listen for the under-sound of the beaten stones grinding together at its foot.

Leaving Niagara on September 2, they travelled to Cincinnati, a 20-hours' journey, where they rested a day; on the 4th another 10 hours took them to Nashville, where they were to meet his sister, Mrs. Scott. Though 11 years his senior, she maintained her vigour and brightness undimmed, as indeed she did to the end of her life, surviving him by a few weeks. As she now stood on the platform at Nashville, Mrs. Huxley, who had never seen her, picked her out from among all the people by her piercing black eyes, so like those of her mother as described in the Autobiographical sketch (*Coll. Ess. i.*).

Nashville, her son's home, had been chosen as the meeting-place by Mrs. Scott, because it was not so far south nor so hot as Montgomery, where she was then living. Nevertheless in Tennessee the heat of the American summer was

I cannot say that I am in the slightest degree impressed by your bigness or your material resources, as such. Size is not grandeur, territory does not make a nation. The great issue, about which hangs a true sublimity, and the terror of overhanging fate, is, what are you going to do with all these things? . . .

The one condition of success, your sole safeguard, is the moral worth and intellectual clearness of the individual citizen. Education cannot give these, but it can cherish them and bring them to the front in whatever station of society they are to be found, and the universities ought to be and may be, the fortresses of the higher life of the nation.

This address was delivered under circumstances of peculiar difficulty. The day before, an expedition had been made to Washington, from which Huxley returned very tired, only to be told that he was to attend a formal dinner and reception the same evening. "I don't know how I shall stand it," he remarked. Going to his room, he snatched an hour or two of rest, but was then called upon to finish his address before going out. It seems that it had to be ready for simultaneous publication in the New York papers. Now the lecture was not written out; it was to be given from notes only. So he had to deliver it *in extenso* to the reporter, who took it down in shorthand, promising to let him have a longhand copy in good time the next morning. It did not come till the last moment. Glancing at it on his way to the lecture theatre, he discovered to his horror that it was written upon "flimsy" from which he would not be able to read it with any success. He wisely gave up the attempt, and made up his mind to deliver the lecture as best he could from memory. The lecture as delivered was very nearly the same as that which he had dictated the night before, but with some curious discrepancies between the two accounts, which, he used to say, occurring as they did in versions both purporting to have been taken down from his lips, might well lead the ingenious critic of the future to pronounce them both spurious, and to declare that the pretended original was never delivered under the circumstances alleged.*

* Cp. the incident at Belfast, p. 444.

on the front limb and three on the hind limb. Judging from the completeness of the series or forms so far, he ventured to indulge in a prophecy.

Thus, thanks to these important researches, it has become evident that, so far as our present knowledge extends, the history of the horse-type is exactly and precisely that which could have been predicted from a knowledge of the principles of evolution. And the knowledge we now possess justifies us completely in the anticipation that when the still lower Eocene deposits, and those which belong to the Cretaceous epoch, have yielded up their remains of ancestral equine animals, we shall find, first, a form with four complete toes and a rudiment of the innermost or first digit in front, with, probably, a rudiment of the fifth digit in the hind foot; while, in still older forms, the series of the digits will be more and more complete, until we come to the five-toed animals, in which, if the doctrine of evolution is well founded, the whole series must have taken its origin.

Seldom has prophecy been sooner fulfilled. Within two months, Professor Marsh had discovered a new genus of equine mammals, *Eohippus*, from the lowest Eocene deposits of the West, which corresponds very nearly to the description given above.

He continues:—

That is what I mean by demonstrative evidence of evolution. An inductive hypothesis is said to be demonstrated when the facts are shown to be in entire accordance with it. If that is not scientific proof, there are no merely inductive conclusions which can be said to be proved. And the doctrine of evolution, at the present time, rests upon exactly as secure a foundation as the Copernican theory of the motions of the heavenly bodies did at the time of its promulgation. Its logical basis is of precisely the same character—the coincidence of the observed facts with theoretical requirements.

He left New York on September 23. "I had a very pleasant trip in Yankee-land," he writes to Professor Baynes, "and did *not* give utterance to a good deal that I am reported to have said there." He reached England in good time for the beginning of his autumn lectures, and his

recollect that this is a sacrifice, and that you should not be surprised if it occasionally happens that you see a biologist apparently trespassing in the region of philosophy or politics; or meddling with human education; because, after all, that is a part of his kingdom which he has only voluntarily forsaken"—how to learn biology, the use of Museums, and above all, the utility of biology, as helping to give right ideas in this world, which "is after all, absolutely governed by ideas, and very often by the wildest and most hypothetical ideas."

This lecture on Biology was first published among the *American Addresses* in 1877.

It was about this time that an extremely Broad Church divine was endeavouring to obtain the signatures of men of science to a document he had drawn up protesting against certain orthodox doctrines. Huxley, however, refused to sign the protest, and wrote the following letter of explanation, a copy of which he sent to Mr. Darwin.

Nov. 18, 1876.

DEAR SIR—I have read the "Protest," with a copy of which you have favoured me, and as you wish that I should do so, I will trouble you with a brief statement of my reasons for my inability to sign it.

I object to clause 2 on the ground long since taken by Hume that the order of the universe such as we observe it to be, furnishes us with the only data upon which we can base any conclusion as to the character of the originator thereof.

As a matter of fact, men sin, and the consequences of their sins affect endless generations of their progeny. Men are tempted, men are punished for the sins of others without merit or demerit of their own; and they are tormented for their evil deeds as long as their consciousness lasts.

The theological doctrines to which you refer, therefore, are simply extensions of generalisations as well based as any in physical science. Very likely they are illegitimate extensions of these generalisations, but that does not make them wrong in principle.

And I should consider it waste of time to "protest" against that which is.

the village church when the parson began to read it; and thinking to gain my father's assent, she turned to him and said graciously, "Now, Mr. Huxley, don't you think I was quite right to mark my disapproval?"

"My dear Lady ——" he replied, "I should as soon think of rising and leaving your table because I disapproved of one of the entrées."

CHAPTER XXXII

1877

IN this year he delivered lectures and addresses on the Geological History of Birds, at the Zoological Society's Gardens, June 7; on "Starfishes and their Allies," at the Royal Institution, March 7; at the London Institution, Dec. 17, on Belemnites (a subject on which he had written in 1864, and which was doubtless suggested anew by his autumn holiday at Whitby, where the Lias cliffs are full of these fossils); at the Anthropological Conference, May 22, on Elementary Instruction in Physiology (*Coll. Ess.* iii. 294), with special reference to the recent legislation as to experiments on living animals; and on Technical Education to the Working Men's Club and Institute, December 1 (*Coll. Ess.* iii. 404): a perilous subject, indeed, considering, as he remarks, that "any candid observer of the phenomena of modern society will readily admit that bores must be classed among the enemies of the human race; and a little consideration will probably lead him to the further admission, that no species of that extensive genus of noxious creatures is more objectionable than the educational bore. . . . In the course of the last ten years, to go back no farther, I am afraid to say how often I have ventured to speak of education; indeed, the only part of this wide region into which, as yet, I have not adventured, is that into which I propose to intrude to-day."

The choice of subject for this address was connected with a larger campaign for the establishment of technical education on a proper footing, which began with his work on the School Board, or was this year brought prominently

before the public by another address delivered at the Society of Arts. The Clothworkers Company had already been assisting the Society of Arts in their efforts for the spread of technical education; and in July 1877 a special committee of the Guilds applied to him, amongst half a dozen others, to furnish them with a report as to the objects and methods of a scheme of technical education. This paper fills sixteen pages in the Report of the Livery Companies' Committee for 1878. The fundamental principles on which he bases his practical recommendations are contained in the following paragraph:—

It appears to me that if every person who is engaged in an industry had access to instruction in the scientific principles on which that industry is based; in the mode of applying these principles to practice; in the actual use of the means and appliances employed; in the language of the people who know as much about the matter as we do ourselves; and lastly, in the art of keeping accounts, Technical Education would have done all that can be required of it.

And his suggestion about buildings was at once adopted by the Committee, namely, that they should be erected at a future date, regard being had primarily rather to what is wanted in the inside than what will look well from the outside.

Now the Guilds formed a very proper body to set such a scheme on foot, because only such wealthy and influential members of the first mercantile city in the world could afford to let themselves be despised and jeered at for professing to teach English manufacturers and English merchants that they needed to be taught; and to spend £25,000 a year towards that end for some time without apparent result.

That they eventually succeeded, is due no little to the careful plans drawn out by Huxley. He may be described as "really the engineer of the City and Guilds Institute; for without his advice," declared one of the leading members, "we should not have known what to have done."

At the same time he warned them against indiscriminate zeal; "though under-instruction is a bad thing, it is not

panies under way in the matter. In the words of Mr. George Howell, M.P.,* it has an additional interest "as indicating the nature of his own epitaph"; as a man "whose highest ambition ever was to uplift the masses of the people and promote their welfare intellectually, socially, and industrially."

4 MARLBOROUGH PLACE, N.W., *Jan.* 2, 1880.

DEAR MR. HOWELL—Your letter is a welcome New Year's gift. There are two things I really care about—one is the progress of scientific thought, and the other is the bettering of the condition of the masses of the people by bettering them in the way of lifting themselves out of the misery which has hitherto been the lot of the majority of them. Posthumous fame is not particularly attractive to me, but, if I am to be remembered at all, I would rather it should be as "a man who did his best to help the people" than by other title. So you see it is no small pleasure and encouragement to me to find that I have been, and am, of any use in this direction.

Ever since my experience on the School Board, I have been convinced that I should lose rather than gain by entering directly into politics. . . . But I suppose I have some ten years of activity left in me, and you may depend upon it I shall lose no chance of striking a blow for the cause I have at heart. I thought the time had come the other day at the Society of Arts, and the event proves I was not mistaken. The animal is moving, and by a judicious exhibition of carrots in front and kicks behind, we shall get him into a fine trot presently. In the meantime do not let the matter rest. . . . The (City) companies should be constantly reminded that a storm is brewing. There are excellent men among them, who want to do what is right, and need help against the sluggards and reactionaries. It will be best for me to be quiet for a while, but you will understand that I am watching for the turn of events.—I am, yours very faithfully,

T. H. HUXLEY.

This summer, too, he delivered a course on Biology for Teachers at South Kensington, and published not only his *American Addresses*, but also the *Physiography*, founded upon the course delivered seven weeks before. The book, of which 3386 copies were sold in the first six weeks, was fruitful in two ways; it showed that a geographical subject

* Who sent it to the *Times* (July 3, 1895) just after Huxley's death.

I am sorry to say I shall have to come here again in Easter week. It is the only time the Lord President is free from his courts, and although we all howled privately, there was no help for it. Whether we finish then or not will depend on the decision of the Government, as to our taking up the case of you troublesome women, who want admission into the University (very rightly too I think). If we have to go into this question it will involve the taking of new evidence and no end of bother. I find my colleagues very reasonable, and I hope some good may be done, that is the only consolation.

I went out with Blackie last evening to dine with the Skeltons, at a pretty place called the Hermitage, about three miles from here. . . . Blackie and I walked home with snow on the ground and a sharp frost. I told you it would turn cold as soon as I got here, but I am none the worse.

It was just the same in April:—

It is quite cold here as usual, and there was ice on the ponds we passed this morning. . . . I am much better lodged than I was last time, for the same thanks to John Bruce, but I do believe that the Edinburgh houses are the coldest in the universe. In spite of a good breakfast and a good fire, the half of me that is writing to you is as cold as charity.

April 4.—We toil at the Commission every day, and don't make any rapid progress. An awful fear creeps over me that we shall not finish this bout.

While he was in Edinburgh for the third time, his attention was called to an article in the *Echo*, the organ of the anti-vivisection party. He writes:—

The *Echo* is pretty. It is one of a long series of articles from the same hand, but I don't think they hurt anybody and they evidently please the writer. For some reason or other they have not attacked me yet, but I suppose my turn will come.

Again:—

Thank you for sending me John Bright's speeches. They are very good, but hardly up to his old mark of eloquence. Some parts are very touching.

His health was improving, as he notes with satisfaction:

Every day this week we have had about four hours of the Commission, and I have dined out four days out of the six.

ceived an enthusiastic account of the proceedings from his son, and wrote to thank Huxley, who replied :—

4 MARLBOROUGH PLACE, *Nov. 21, 1877.*

MY DEAR DARWIN—Nothing ever gave me greater pleasure than the using the chance of speaking my mind about you and your work which was afforded me at the dinner the other night. I said not a word beyond what I believe to be strictly accurate : and, please Sir, I didn't sneer at anybody. There was only a little touch of the whip at starting, and it was so tied round with ribbons that it took them some time to find out where the flick had hit.

T. H. HUXLEY.

He writes to his wife :—

I will see if I can recollect the speech. I made a few notes sitting in Dewar's room before the dinner. But as usual I did not say some things I meant to say, and said others that came up on the spur of the moment.

And again :—

Please I didn't say that Réaumur was the other greatest scientific man since Aristotle. But I said that in a certain character of his work he was the biggest man between Aristotle and Darwin. I really must write out an "authorised version" of my speech. I hear the Latin oration is to be in *Nature* this week, and Lockyer wanted me to give him the heads of my speech, but I did not think it would be proper to do so, and refused. I have written out my speech as well as I can recollect it. I do not mind any friend seeing it, but you must not let it get about as the dinner was a private one.

The notes of his speech run as follows :—

MR. PRESIDENT—I rise with pleasure and with alacrity to respond to the toast which you have just proposed, and I may say that I consider one of the greatest honours which have befallen me, to be called upon to represent my distinguished friend Mr. Darwin upon this occasion. I say to represent Mr. Darwin, for I cannot hope to personate him, or to say all that would be dictated by a mind conspicuous for its powerful humility and strong gentleness.

Mr. Darwin's work had fully earned the distinction you have to-day conferred upon him four and twenty years ago; but I doubt not that he would have found in that circum-

opinion, that from Aristotle's great summary of the Biological knowledge of his time down to the present day, there is nothing comparable to the *Origin of Species*, as a connected survey of the phenomena of life permeated and vivified by a central idea. In remote ages the historian of science will dwell upon it as the starting-point of the Biology of his present and our future.

My friend Dr. Humphry has adverted to somebody about whom I know nothing, who says that the exact and critical studies pursued in this University are ill-calculated to preserve a high tone of mind.

I presume that this saying must proceed from some one wholly unacquainted with Cambridge. Whoever he may be, I beg him, if he can, to make the acquaintance of Charles Darwin.

In Mr. Darwin's name I beg leave to thank you for the honour you have done him.

It happened that the quadrennial election of a Lord Rector at St. Andrews University fell in this year, and on behalf of a number of students, Huxley received a telegram from his son, now newly entered at St. Andrews, asking him to stand. He writes to his wife:—

That boy of yours has just sent me a telegram, which I enclose. I sent back message to say that as a Commissioner on the Scotch Universities I could not possibly stand. The cockerel is beginning to crow early. I do believe that to please the boy I should have assented to it if it had not been for the R. Commission.

Apropos of controversies (November 23)

We had a grand discussion at the Royal Society last night between Tyndall and Burdon Sanderson. The place was crammed, and we had a late sitting. I'm not sure, however, that we had got much further at the end than at the beginning, which is a way controversies have.

The following story is worth recording, as an illustration not only of the way in which Huxley would give what help was in his power to another man of science in distress, but of the ready aid proffered on this, as on many other occasions, by a wealthy northern merchant who was interested in science. A German scientific worker in England, whom we will call H., had fallen into distress, and applied to him

much better for you, and I believe for Mrs. Morley, than the atmosphere of the melancholy main, the effects of which on the human constitution have been so well expounded by that eminent empiric, Dr. Dizzy.

Anyhow, I wish we could see something of you now and then.—Ever yours very faithfully, T. H. HUXLEY.

Darwin got his degree with great *éclat* on Saturday. I had to return thanks for his health at the dinner of the Philosophical Society; and oh! I chaffed the dons so sweetly.

4 MARLBOROUGH PLACE, N.W., *Nov. 27, 1877.*

MY DEAR MORLEY—You shall have both the articles—if it is only that I may enjoy the innocent pleasure of Knowles' face when I let him know what has become of them.

Stormy ocean, forsooth! I back the storm and rain through which I came home to-night against anything London-super-mare has to show.

I will send the MS. to Virtue as soon as it is in a reasonable state.—Ever yours very faithfully, T. H. HUXLEY.

4 MARLBOROUGH PLACE, N.W., *Jan. 8, 1878.*

MY DEAR MORLEY—Many thanks for the cheque. In my humble judgment it is quite as much as the commodity is worth.

It was a great pleasure to us all to have you with us on New Year's Day. My wife claims it as her day, and I am not supposed to know anything about the guests except Spencer and Tyndall. None but the very elect are invited to the sacred feast—so you see where you stand among the predestined who cannot fall away from the state of grace.

I have not seen Spencer in such good form and good humour combined for an age.

I am working away at Harvey, and will send the MS. to Virtue's as soon as I am sufficiently forward.—Ever yours very faithfully, T. H. HUXLEY.

4 MARLBOROUGH PLACE, *Dec. 9, 1877.*

MY DEAR TYNDALL—I am so sorry to have been out when Mrs. Tyndall called to-day. By what we heard at the *x* on Thursday, I imagined you were practically all right again, or I should have been able to look after you to-day.

But what I bother you with this note for is to beg you not to lecture at the London Institution to-morrow, but to let me

change days with you, and so give yourself a week to recover. And if you are seedy, then I am quite ready to give them another lecture on the Hokyptotamus or whatever else may turn up.

But don't go and exert yourself in your present condition. These severe colds have often nothing very tangible about them, but are not to be trifled with when folks are past fifty.

Let me have an answer to say that I may send a telegram to Nicholson first thing to-morrow morning to say that I will lecture *vice* you. My "bottled life," as Hutton calls it in the *Spectator* * this week, is quite ready to go off.

Now be a sane man and take my advice.—Ever yours very faithfully,
T. H. HUXLEY.

* The *Spectator* for Dec. 8, 1877, began an article thus:—"Professor Huxley delivered a very amusing address last Saturday at the Society of Arts, on the very unpromising subject of technical education; but we believe that if Professor Huxley were to become the President of the Social Science Association, or of the International Statistical Congress, he would still be amusing, so much bottled life does he infuse into the driest topic on which human beings ever contrived to prose."

CHAPTER XXXIII

1878

THE year 1878 was the tercentenary of Harvey's birth, and Huxley was very busy with the life and work of that great physician. He spoke at the memorial meeting at the College of Physicians (July 18), he gave a lecture on Harvey at the Royal Institution on January 25, afterwards published in *Nature* and the *Fortnightly Review*, and intended to write a book on him in a projected *English Men of Science* series (see p. 536).

I am very glad you like "Harvey" (he writes to Prof. Baynes on Feb. 11). He is one of the biggest scientific minds we have had. I expect to get well vilipended not only by the anti-vivisection folk, for the most of whom I have a hearty contempt, but *apropos* of Bacon. I have been oppressed by the humbug of the "Baconian Induction" all my life, and at last *the worm has turned*.

Now in this lecture he showed that Harvey employed vivisection to establish the doctrine of the circulation of the blood, and furthermore, that he taught this doctrine before the *Novum Organum* was published, and that his subsequent *Exercitatio* displays no trace of being influenced by Bacon's work. After glancing at the superstitious reverence for the "Baconian Induction," he pointed out Bacon's ignorance of the progress of science up to his time, and his inability to divine the importance of what he knew by hearsay of the work of Copernicus, or Kepler, or Galileo; of Gilbert, his contemporary, or of Galen; and wound up by quoting Ellis's severe judgment of Bacon in the General Preface to the *Philosophic Works*, in Spedding's classical edition (p. 38):—

mony to the influence of Huxley's writings even on his elder contemporaries.

FROM JAMES SPEDDING

Feb. 1, 1878.

. . . When you admit that you study Bacon with a *prejudice*, you mean of course an unfavourable opinion previously formed on sufficient grounds. Now I am myself supposed to have studied him with a prejudice the other way: but this I cannot admit, in any sense of the word; for when I first made his acquaintance I had no opinion or feeling about him at all—more than the ordinary expectation of a young man to find what he is told to look for. My earliest impression of his character came probably from Thomson—whose portrait of him, except as touched and softened by the tenderer hand of “the sweet-souled poet of the Seasons,” did not differ from the ordinary one. It was not long indeed before I did begin to form an opinion of my own; one of those *after*-judgments which are liable to be mistaken for prejudices by those who judge differently, and which, being formed, do, no doubt, tell upon the balance. For it was not long before I found myself indebted to him for the greatest benefit probably that any man, living or dead, can confer on another. In my school and college days I had been betrayed by an ambition to excel in themes and declamations into the study, admiration, and imitation of the rhetoricians. In the course of my last long vacation—the autumn of 1830—I was inspired with a new ambition, namely, to think justly about everything which I thought about at all, and to act accordingly; a conviction for which I cannot cease to feel grateful, and which I distinctly trace to the accident of having in the beginning of that same vacation given two shillings at a second-hand bookstall for a little volume of Dove's classics, containing the *Advancement of Learning*. And if I could tell you how many superlatives I have since that time degraded into the positive; how many innumerables and infinites I have replaced by counted numbers and estimated quantities; how many assumptions, important to the argument in hand, I have withdrawn because I found on more consideration that the fact might be explained otherwise; and how many effective epithets I have discarded when I found that I could not fully verify them; you would think it no less than just that I should claim for myself and concede to others the right of being judged by the last edition rather than the first. That a persistent en-

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deavour to free myself from what you regard as Bacon's characteristic vice should have been the fruit of a desire to follow his example, will seem strange to you, but it is fact. Perhaps you will think it not less strange, but it is my real belief, that if your own writings had been in existence and come in my way at the same critical stage of my moral and mental development, they would have taught me the same lesson and inspired me with the same ambition; for in that particular (if I may say it without offence) I look upon you *both* as eminent examples of the *same* virtue.

To the lecture he refers once more in a letter to Mr. John Morley. The political situation touched on in this and the next letter, is that of the end of the Russo-Turkish war and the beginning of the Afghan war.

SCIENCE SCHOOLS, SOUTH KENSINGTON,
Feb. 7, 1878.

MY DEAR MORLEY—Many thanks for the cheque, and still more for the good word for the article.* I knew it would "draw" Hutton, and his ingenuity has as usual made the best of the possibilities of attack. I am glad to find, however, that he does not think it expedient to reiterate his old story about the valuelessness of vivisection in the establishment of the doctrine of the circulation.

I hear that that absurd creature R—— goes about declaring that I have made all sorts of blunders. Could not somebody be got to persuade him to put what he has to say in black and white?

Controversy is as abhorrent to me as gin to a reclaimed drunkard; but oh dear! it would be so nice to squelch that pompous impostor.

I hope you admire the late aspects of the British Lion. His tail goes up and down from the intercrural to the stiffly erect attitude per telegram, while his head is sunk in the windbag of the House of Commons.

I am beginning to think that a war would be a good thing if only for the inevitable clean sweep of all the present governing people which it would bring about.—Ever yours very faithfully,

T. H. HUXLEY.

* On Harvey.

TO HIS ELDEST DAUGHTER

SCIENCE SCHOOLS, SOUTH KENSINGTON,
Dec. 7, 1878.

DEAREST JESS—You are a badly used young person—you are; and nothing short of that conviction would get a letter out of your still worse used Pater the *bête noire* of whose existence is letter-writing.

Catch me discussing the Afghan question with you you little pepper pot. No, not if I know it. Read Fitzjames Stephen's letter in the *Times*, also Bartle Frere's memorandum, also Napier of Magdala's memo. Them's my sentiments.

Also read the speech of Lord Hartington on the address. He is a man of sense like his father, and you will observe that he declares that the Government were perfectly within their right in declaring war without calling Parliament together. . . .

If you had lived as long as I have and seen as much of men, you would cease to be surprised at the reputations men of essentially commonplace powers—aided by circumstances and some amount of cleverness—obtain.

I am as strong for justice as any one can be, but it is real justice, not sham conventional justice which the sentimentalists howl for.

At this present time real justice requires that the power of England should be used to maintain order and introduce civilisation wherever that power extends.

The Afghans are a pack of disorderly treacherous blood-thirsty thieves and caterans who should never have been allowed to escape from the heavy hand we laid upon them, after the massacre of twenty thousand of our men, women (and) children in the Khoord Cabul Pass thirty years ago.

We have let them be, and the consequence is they now lend themselves to the Russians, and are ready to stir up disorder and undo all the good we have been doing in India for the last generation.

They are to India exactly what the Highlanders of Scotland were to the Lowlanders before 1745; and we have just as much right to deal with them in the same way.

I am of opinion that our Indian Empire is a curse to us. But so long as we make up our minds to hold it, we must also make up our minds to do those things which are needful to hold it effectually, and in the long-run it will be found that so doing is

remained but to put a few finishing touches and to write detailed descriptions of the plates.

Letters to W. K. Parker and Professor Haeckel touch on this part of his work; the former, indeed, offering a close parallel to a story, obviously of the same period, which the younger Parker tells in his reminiscences, to illustrate the way in which he would be utterly engrossed in a subject for the time being. Jeffery Parker, while demonstrator of biology, came to him with a question about the brain of the codfish at a time when he was deep in the investigation of some invertebrate group. "Codfish?" he replied, "that's a vertebrate, isn't it? Ask me a fortnight hence, and I'll consider it."

4 MARLBOROUGH PLACE, *Sept.* 25, 1878.

MY DEAR PARKER—As far as I recollect *Ammocætes* is a vertebrated animal—and I ignore it.

The paper you refer to was written by my best friend—a carefulish kind of man—and I am sure that he saw what he says he saw, as if I had seen it myself.

But what the fact may mean and whether it is temporary or permanent—is thy servant a dog that he should worry himself about other things with backbones? Not if I know it.

Churchill has got over a whole batch of the American edition of the *Vertebrata*, so I have a respite. Mollusks are far more interesting—bugs sweeter—while the dinner crayfish hath no parallel for intense and absorbing interest in the three kingdoms of Nature.

What saith the Scripture? "Go to the ANT thou sluggard." In other words, study the Invertebrata.—Ever yours very faithfully,
T. H. HUXLEY.

[Sketch of a vast winged ant advancing on a midget, and saying, as it looks through a pair of eyeglasses, "well, really, what an absurd creature!!"]

4 MARLBOROUGH PLACE, LONDON,
April 28, 1878.

MY DEAR HAECKEL—Since the receipt of your letter three months ago, I have been making many inquiries about *Medusæ* for you, but I could hear of none—and so I have delayed my reply, until I doubt not you have been blaspheming my apparent neglect.

give yourself any trouble about him, I need not say I shall be very grateful for any notice you may take of him.

I am giving him as much independence of action as possible, in order that he may learn to take care of himself.

Now that is enough about my children. Yours must yet be young—and you have not yet got to the marriage and university stage—which I assure you is much more troublesome than the measles and chicken-pox period.

My wife unites with me in kindest remembrances and good wishes.—Ever yours very faithfully, T. H. HUXLEY.

An outbreak of diphtheria among his children made the spring of 1878 a time of overwhelming anxiety. How it told upon his strong and self-contained chief is related by T. J. Parker—"I never saw a man more crushed than he was during the dangerous illness of one of his daughters, and he told me that, having then to make an after-dinner speech, he broke down for the first time in his life, and for one painful moment forgot where he was and what he had to say." This was one of the few occasions of his absence from College during the seventies. "When, after two days, he looked in at the laboratory," writes Professor Howes, "his dejected countenance and tired expression betokened only too plainly the intense anxiety he had undergone."

The history of the outbreak was very instructive. Huxley took a leading part in organising an enquiry and in looking into the matter with the health officer. "As soon as I can get all the facts together," he writes on Dec. 10, "I am going to make a great turmoil about our outbreak of diphtheria—and see whether I cannot get our happy-go-lucky local government mended." As usual, the epidemic was due to culpable negligence. In the construction of some drains, too small a pipe was laid down. The sewage could not escape, and flooded back in a low-lying part of Kilburn. Diphtheria soon broke out close by. While it was raging there, a St. John's Wood dairyman running short of milk, sent for more to an infected dairy in Kilburn. Every house which he supplied that day with Kilburn milk was attacked with diphtheria.

end at the seaside, Professor Marsh was not entirely neglected. He writes in his *Recollections* (p. 6):—

How kind Huxley was to everyone who could claim his friendship, I have good cause to know. Of the many instances which occur to me, one will suffice. One evening in London at a grand annual reception of the Royal Academy, where celebrities of every rank were present, Huxley said to me, "When I was in America, you showed me every extinct animal that I had read about, or even dreamt of. Now, if there is a single living lion in all Great Britain that you wish to see, I will show him to you in five minutes." He kept his promise, and before the reception was over, I had met many of the most noted men of England, and from that evening, I can date a large number of acquaintances, who have made my subsequent visits to that country an ever-increasing pleasure.

As for his summer occupations, he writes to his eldest daughter on July 2:—

No, young woman, you don't catch me attending any congresses I can avoid, not even if F. is an artful committee-man. I must go to the British Association at Dublin—for my sins—and after that we have promised to pay a visit in Ireland to Sir Victor Brooke. After that I must settle myself down in Penmaenmawr and write a little book about David Hume—before the grindery of the winter begins.

The meeting of the British Association took place this year in the third week of August at Dublin. Huxley gave an address in the Anthropological subsection,* and on the 20th received the honorary degree of LL.D. from Dublin University, the Public Orator presenting him in the following words:—

Præsentō vobis Thomam Henricum Huxley—hominem vere physicum—hominem facundum, lepidum, venustum—eundem autem nihil (philosophia modo sua lucem præferat) reformidantem—ne illud quidem Ennianum,

Simia quam similis, turpissima bestia, nobis.

* "Informal Remarks on the Conclusions of Anthropology," *B. A. Report*, 1878, pp. 573–578.

The extract above given contains the first reference to the book on Hume,* written this summer as a holiday occupation at Penmaenmawr. The speed at which it was composed is remarkable, even allowing for his close knowledge of the subject, acquired many years before. Though he had been "picking at it" earlier in the summer, the whole of the philosophical part was written during September, leaving the biographical part to be done later.

The following letters from Marlborough Place show him at work upon the book:—

March 31, 1878.

MY DEAR MORLEY—I like the notion of undertaking your Hume book, and I don't see why I should not get it done this autumn. But you must not consider me pledged on that point, as I cannot quite command my time.

Tulloch sent me his book on Pascal. It was interesting as everything about Pascal must be, but Tulloch is not a model of style.

I have looked into Bruton's book, but I shall now get it and study it. Hume's correspondence with Rousseau seems to me typical of the man's sweet, easy-going nature. Do you mean to have a portrait of each of your men? I think it is a great comfort in a biography to get a notion of the subject in the flesh.

I have rather made it a rule not to part with my property in my books—but I daresay that can be arranged with Macmillan. Anyhow I shall be content to abide by the general arrangement if you have made one.

We have had a bad evening. Clifford has been here, and he is extremely ill—in fact I fear the worst for him.

It is a thousand pities, for he has a fine nature all round, and time would have ripened him into something very considerable. We are all very fond of him.—Ever yours very faithfully,
T. H. HUXLEY.

July 6, 1878.

MY DEAR MORLEY—Very many thanks for Diderot. I have made a plunge into the first volume and found it very interesting. I wish you had put a portrait of him as a frontispiece. I have seen one—a wonderful face, something like Goethe's.

* In the "English Men of Letters" series, edited by Mr. John Morley.

I am picking at Hume at odd times. It seems to me that I had better make an analysis and criticism of the "Inquiry," the backbone of the essay—as it touches all the problems which interest us most just now. I have already sketched out a chapter on Miracles, which will, I hope, be very edifying in consequence of its entire agreement with the orthodox arguments against Hume's *a priori* reasonings against miracles.

Hume wasn't half a sceptic after all. And so long as he got deep enough to worry Orthodoxy, he did not care to go to the bottom of things.

He failed to see the importance of suggestions already made both by Locke and Berkeley.—Ever yours very faithfully,

T. H. HUXLEY.

Sept. 30, 1878.

MY DEAR MORLEY—Praise me! I have been hard at work at Hume at Penmaenmawr, and I have got the hard part of the business—the account of his philosophy—blocked out in the bodily shape of about 180 pages foolscap MS.

But I find the job as tough as it is interesting. Hume's diamonds, before the public can see them properly, want a proper setting in a methodical and consistent shape—and that implies writing a small psychological treatise of one's own, and then cutting it down into as unobtrusive a form as possible.

So I am working away at my draught—from the point of view of an æsthetic jeweller.

As soon as I get it into such a condition as will need only verbal trimming, I should like to have it set up in type. For it is a defect of mine that I can never judge properly of any composition of my own in manuscript.

Moreover (don't swear at this wish) I should very much like to send it to you in that shape for criticism.

The Life will be an easy business. I should like to get the book out of hand before Christmas, and will do so if possible. But my lectures begin on Tuesday, and I cannot promise.—Ever yours very faithfully,

T. H. HUXLEY.

Oct. 21, 1878.

MY DEAR MORLEY—I have received slips up to chap. ix. of Hume, and so far I do not think (saving your critical presence) that there will be much need of much modification or interpolation.

I have made all my citations from a 4-vol. edition of Hume,

4 MARLBOROUGH PLACE, N.W., *Nov.* 5, 1878.

MY DEAR MORLEY—"Davie's" philosophy is now all in print, and all but a few final pages of his biography.

So I think the time has come when that little critical symposium may take place.

Can you come and dine on Tuesday next (12) at 7, or if any day except Wednesday 15th, next week, will suit you better, it will do just as well for me. There will be nobody but my wife and daughters, so don't dress.—Ever yours very faithfully,

T. H. HUXLEY.

P.S.—Will you be disgusted if in imitation of the "English Men of Letters" I set a-going an "English Men of Science." Few people have any conception of the part Englishmen have played in science, and I think it would be both useful and interesting to bring the truth home to the English mind.

I had about three thousand people to hear me on Saturday at Manchester, and it would have done you good to hear how they cheered at my allusion to personal rule. I had to stop and let them ease their souls.

Behold my P.S. is longer than my letter. It's the strong feminine element in my character oozing out. "Desinit in piscem" though, and a mighty queer fish too.

4 MARLBOROUGH PLACE, *Jan.* 12, 1879.

DEAR LECKY—I am very much obliged for your suggestion about the note at p. 9. I am ashamed to say that though the eleven day correction was familiar enough to me, I had never thought about the shifting of the beginning of the year till you mentioned it. It is a law of nature, I believe, that when a man says what he need not say he is sure to blunder. The note shall go out.

All I know about Sprat is as the author of a dull history of the Royal Society, so I was surprised to meet with Hume's estimate of him.

No doubt about the general hatred of the Scotch, but you will observe that I make Millar responsible for the peace-making assurance.

What you said to me in conversation some time ago led me to look at Hume's position as a moralist with some care, and I quoted the passage at p. 206 that no doubt might be left on the matter.

The little book threatened to grow to an undue length, and

I forget whether I asked you before. From all I hear of him I expect he would do No. 6 very well. I have written to Adamson by this post.

I shall get off with Harvey and Darwin to my share.—
Ever yours very faithfully, T. H. HUXLEY.

4 MARLBOROUGH PLACE, N.W., Dec. 26, 1878.

MY DEAR ROSCOE—I was very loth to lump the chemists together, but Max was very strong about not having too many books in the series; and on the other hand, I had my doubts how far the chemists were capable of “dissociation” without making the book too technical.

But I do not regard the present arrangement as unalterable, and if you think the early chemists and the later chemists would do better in two separate groups, the matter is quite open to consideration.

Maxwell says he is overdone with work already, and altogether declines to take anything new. I shall have to look about me for a man to do the Physikers.

Of course Adamson will have to take in a view of the science of the middle ages. That will be one of the most interesting parts of the book, and I hope he will do it well. I suppose he knows his Dante.

The final cause of boys is to catch something or other. I trust that yours is demeaning himself properly.—Ever yours very faithfully, T. H. HUXLEY.

4 MARLBOROUGH PLACE, Dec. 1878.

MY DEAR TYNDALL—I consider your saying the other evening that you would see “anyone else d——d first,” before you would assent to the little proposal I made to you, as the most distinct and binding acceptance you are capable of. You have nothing else to swear by, and so you swear at everybody but me when you want to pledge yourself.

It will release me of an immense difficulty if you will undertake R. Boyle and the Royal Society (which of course includes Hooke); and the subject is a capital one.

The book should not exceed about 200 pages, and you need not be ready before this time next year. There could not be a more refreshing piece of work just to enliven the *dolce far niente* of the Bel Alp. (That is quite *à la* Knowles, and I begin to think I have some faculty as an editor.)

is a general prostration—very sadly obvious when he was with us on Sunday—which, as I understand, rather renders him specially obnoxious to a sudden and rapid development of the lung disease than is itself to be feared.

It was agreed that they should go at once to Gibraltar by the P. and O., and report progress when he gets there. If strong enough he is to go on a cruise round the Mediterranean, and if he improves by this he is to go away for a year to Bogota (in S. America) which appears to be a favourable climate for such cases as his.

If he gets worse he can but return. I have done my best to impress upon him and his wife the necessity of extreme care, and I hope they will be wise.

It is very pleasant to find how good and cordial everybody is, helpful in word and deed to the poor young people. I know it will rejoice the cockles of your generous old heart to hear it.

As for yourself, I trust you are mending and allowing yourself to be taken care of by your household goddess.

With our united love to her and yourself,—Ever yours faithfully,

T. H. HUXLEY.

I sent your cheque to Yeo.

May, 1878.

MY DEAR TYNDALL—You were very much wanted on Saturday, as your wife will have told you, but for all that I would not have had you come on any account. You want a thorough long rest and freedom from excitement of all sorts, and I am rejoiced to hear that you are going out of the hurly-burly of London as soon as possible; and, not to be uncivil, I do hope you will stay away as long as possible, and not be deluded into taking up any exciting pursuit as soon as you feel lively again among your mountains.

Pray give up Dublin. If you don't, I declare I will try if I have enough influence with the council to get you turned out of your office of Lecturer, and superseded.

Do seriously consider this, as you will be undoing the good results of your summer's rest. I believe your heart is as sound as your watch was when you went on your memorable slide,* but if you go slithering down avalanches of work and worry you can't always expect to pick up "the little creature" none

* On the Piz Morteratsch: *Hours of Exercise in the Alps*, by J. Tyndall, ch. xix.

the worse. The apparatus is by one of the best makers, but it has been some years in use, and can't be expected to stand rough work.

You will be glad to hear that we had cheerier news of Clifford on Saturday. He was distinctly better, and setting out on his Mediterranean voyage.—Ever yours very faithfully,

T. H. HUXLEY.

A birthday letter to his son concludes the year:

4 MARLBOROUGH PLACE, N. W., *Dec.* 10, 1878.

Your mother reminds me that to-morrow is your eighteenth birthday, and though I know that my "happy returns" will reach you a few hours too late I cannot but send them.

You are touching manhood now, my dear laddie, and I trust that as a man your mother and I may always find reason to regard you as we have done throughout your boyhood.

The great thing in the world is not so much to seek happiness as to earn peace and self-respect. I have not troubled you much with paternal didactics—but that bit is "ower true" and worth thinking over.

the first of these is the fact that the British Empire is the most powerful in the world, and that it is the only one which has not yet reached its maximum extent.

The second fact is that the British Empire is the only one which has not yet reached its maximum extent. The third fact is that the British Empire is the only one which has not yet reached its maximum extent.

The fourth fact is that the British Empire is the only one which has not yet reached its maximum extent.

The fifth fact is that the British Empire is the only one which has not yet reached its maximum extent.

The sixth fact is that the British Empire is the only one which has not yet reached its maximum extent.

The seventh fact is that the British Empire is the only one which has not yet reached its maximum extent.

The eighth fact is that the British Empire is the only one which has not yet reached its maximum extent.

The ninth fact is that the British Empire is the only one which has not yet reached its maximum extent.

The tenth fact is that the British Empire is the only one which has not yet reached its maximum extent.

(see pp. 349–530, and ii., 287), is the rejection of the prevalent theory that community of language implies community of race, Schleicher, indeed, asserting that language is a more constant character than cranial peculiarities. We talk of Latin and Teutonic races, of Celtic and English, dividing them, in fact, by their language, with no more accuracy than if an archæologist of the future were to call the negroes of Hayti a European stock because they spoke French. “Community of language testifies to close contact between the people who speak the language, but to nothing else.”

In this connection he writes to Professor Max Müller, on June 15, 1865:

MY DEAR SIR—I beg your acceptance of the number of the *Fortnightly Review* containing my article on Ethnology, which accompanies this note.

I lost no time, on Monday, in referring to “Christianity and Mankind,” and the perusal of your chapter on Ethnology *vs.* Phonology led me profoundly to regret that I had not been able to avail myself of the aid of so powerful an ally.

But if you wish to continue to pull one way and I the other, I have hopes we shall be able to get Ethnology and Phonology apart in time.

Ever, my dear sir, very faithfully yours,

T. H. HUXLEY.

Page 305.—Governor Eyre, however, though recalled, escaped condemnation on the ground that he was honestly convinced of Gordon’s guilt and had acted without legal “malice.” The Jamaica Committee raised the question in Parliament, but the Government refused to prosecute him for murder, though ready to revise the sentences of those he had punished and to give them compensation. In these circumstances the House contented itself with a resolution deploring the excessive punishments, and the subsequent efforts of the Jamaica Committee to obtain a conviction in a court of law were fruitless. The magistrates of Shropshire, where Eyre was then living, refused to issue a warrant against the ex-Governor; and in London, grand juries ignored the bills against Colonel Nelson and one of the “preposterous subalterns” in 1867, and against Eyre himself in 1868. The latter spent the remainder of his life in retirement, dying in November, 1901, at the age of eighty-six.

